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ABSTRACT

Five papers which criticize Charles Murray's book, "Losing Ground: American Social Policy 1950-1980" are presented in this report. In general, the papers dispute Murray's thesis that the poor did not benefit from social policies but were, instead, substantially harmed by these programs. The papers (and their authors) are: (1) "Charles Murray and the Family" (Sara McLanahan); (2) "Comments on Murray's Analysis of the Impact of the War on Poverty on the Labor Market Behavior of the Poor" (Glen Cain); (3) "Comments on Schooling" (Michael Olneck); (4) "The 1965-1970 Crime Increase as Seen by Charles Murray: A Critique" (Irving Piliavin); and (5) "Social Programs--A Partial Solution to, but Not a Cause of Poverty: An Alternative to Charles Murray's View" (Sheldon Danziger and Peter Gottschalk). (KH)

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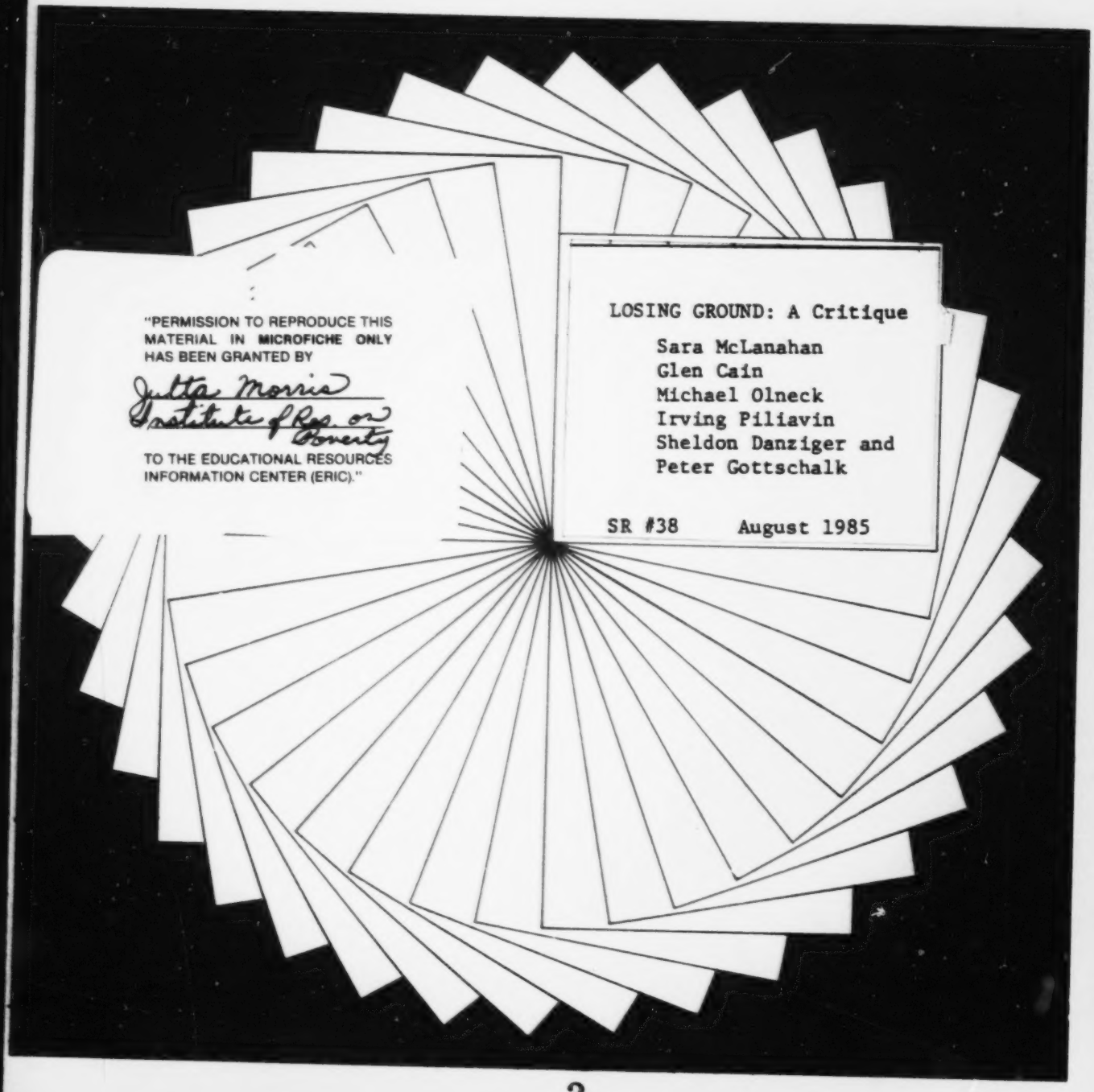
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SR #38 August 1985

Institute for Research on Poverty
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Losing Ground: A Critique

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Preface

Charles Murray's book Losing Ground: American Social Policy 1950-1980 (New York: Basic Books, 1984) has been hailed by many as the coup de grace of the Great Society efforts to aid the poor. Murray is credited with demonstrating not only that the poor did not benefit from social policies but that they were substantially harmed by the very programs designed to help them.

Institute for Research on Poverty social scientists who disagree with Murray's analyses and his conclusions have assembled their critiques in this report. The contribution by Sheldon Danziger and Peter Gottschalk appeared in somewhat different form in Challenge, May-June 1985.

Numbers in parentheses refer to pages in Murray's book.

Elizabeth Uhr provided valuable editorial assistance in the preparation of this Special Report.

Sara McLanahan: Charles Murray and the Family

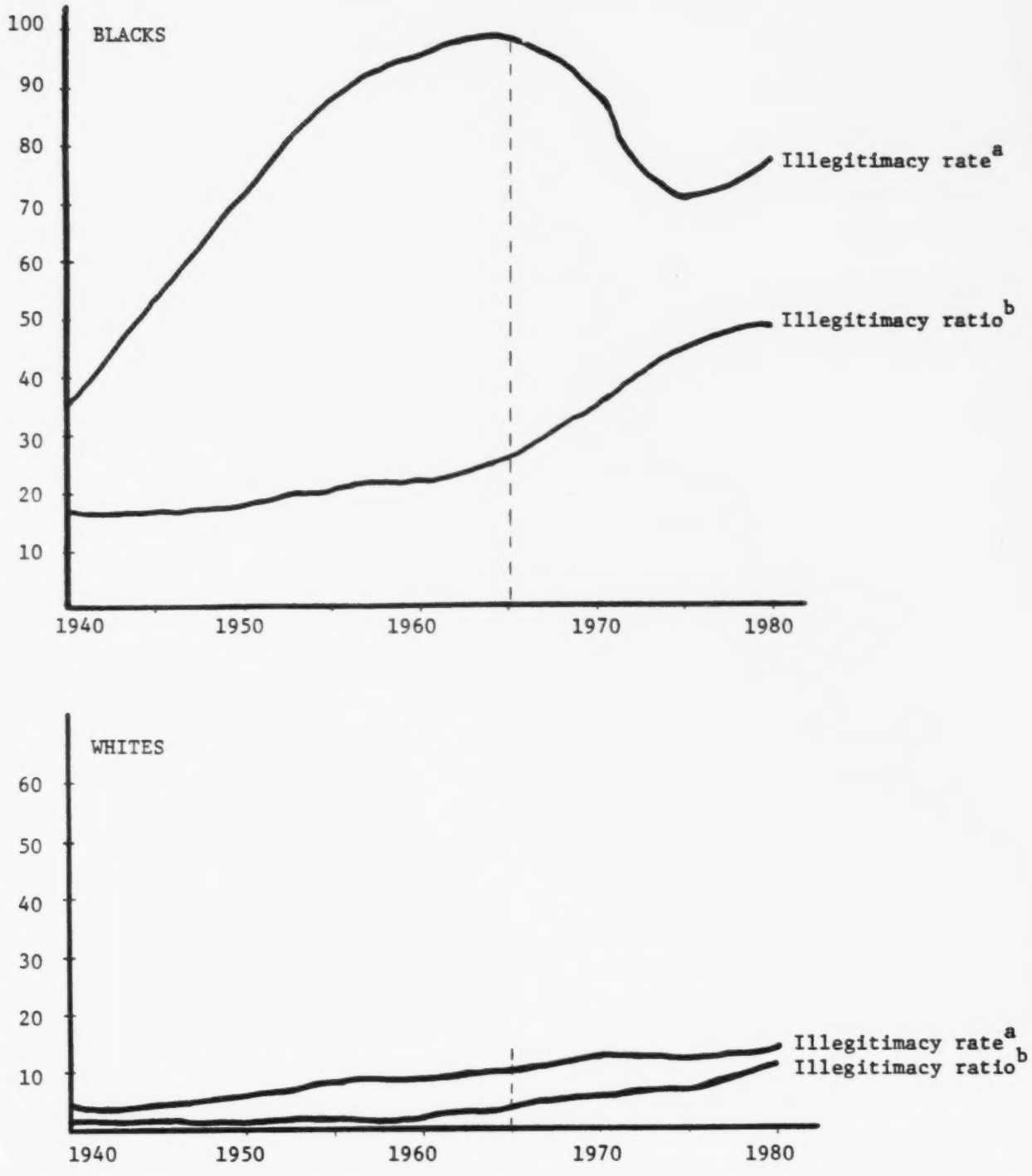
Dr. Murray argues that the growth of female-headed families during the past two decades was a response to increases in the generosity and availability of welfare programs. His solution to the problem, which he offers at the end of his book, is to eliminate welfare benefits to single mothers and their children. He claims this will "drastically reduce births to single teenage girls . . . reverse the trendline in the breakup of poor families . . . [and] increase the upward socioeconomic mobility of poor families" (p. 227).

Murray's claim for the importance of welfare programs in producing the growth of female-headed families is based primarily on his analysis of the trend in illegitimate births. In Chapter 9 he presents a figure showing the trend in the illegitimacy ratio for blacks and whites between 1950 and 1980. Both trendlines increase substantially during the sixties and most of the seventies. Moreover, the black line jumps sharply in the mid-1960s, coinciding almost perfectly with the initiation of the Great Society programs. Murray places a good deal of emphasis on the jumps in the two trends and argues that the increase in welfare has caused the increase in illegitimacy.

All of the evidence presented by Murray is based on the illegitimacy ratio, which is the ratio of nonmarital births to all live births. If, however, we look at the trend in the illegitimacy <u>rate</u>, which is the ratio of nonmarital births to the total number of women at risk for such an event (single women between the ages of 15 and 44), a very different picture emerges. The trends in illegitimacy rates for blacks and whites are presented in Figure 1 below.

Figure 1

Comparison of Illegitimacy Statistics



Sources: Monthly Vital Statistic Report, Vol. 32, No. 9, December 1983; Vital and Health Statistics, Series 21, No. 36, 1980.

Vertical dashed line denotes beginning of acceleration in federal welfare program expenditures

[a] Illegitimacy rate: nonmarital births per 1000 nonmarried women, aged 15-44.
[b] Illegitimacy ratio: ratio of nonmarital births per 100 live births.

For black women, the illegitimacy rate rose sharply between 1945 and 1960, leveled off between 1960 and 1965, and began to decline after 1965. For whites, it increased during the late sixties, declined during the early seventies and rose again during the late seventies. Based on these trends, we would have to conclude that the rise in the illegitimacy rate among blacks began well before the Great Society and actually declined during the most of the time that welfare benefits were increasing. Among whites, the initial trends were consistent with the Murray argument. But the illegitimacy rate declined during the early seventies when welfare benefits were increasing (if we count food stamps and Medicaid) and went up again during the late seventies, when benefits were declining. Murray is aware of the discrepancy between the two trends and says so at the beginning of his discussion. Having acknowledged the difference, however, he focuses exclusively on the illegitimacy ratio which, of course, is consistent with his explanation.

Why, one might ask, is the illegitimacy rate any more valid than the ratio? And how does one decide which statistic to use for examining a particular trend? The answer is as follows: If one is interested in describing the experience of a particular group, e.g., children, the ratio is the appropriate statistic. It tells us what proportion of children are born to nonmarried women and presumably what proportion are at high risk for being in poverty. If, however, one wants to talk about behaviors or propensities, the <u>rate</u>, not the <u>ratio</u>, is the appropriate statistic.[1] The illegitimacy rate tells us what proportion of nonmarried women are having children out of wedlock. If the propensity of nonmarried women to have children is increasing, it should be reflected by an increase in the illegitimacy rate.

In his chapter on the family, Dr. Murray is clearly talking about changes in behavior. His story of Phyllis and Harold--the imaginary couple who must choose between marriage on the one hand and staying single and going on welfare on the other hand--is a story about changes in decisions and behaviors. According to his argument, the number of female-headed families grew during the late sixties primarily because a greater proportion of pregnant Phyllises were deciding not to marry and to have their babies anyway than was the case in 1960. Hence, the proper statistic for testing Murray's theory is the illegitimacy rate. If a greater proportion of the pregnant Phyllises were not marrying in 1970 than in 1960, the illegitimacy rate should have gone up, all else being equal.

Why are the trends in the two statistics so different? First, it is clear that the illegitimacy ratio jumped during the mid-sixties, not because of an increase in the rate of births to single women, but because of a dramatic decrease in marriage rates and a decline in the fertility of married couples. Now Murray might argue that his theory is about the decline in marriage--the decision of Phyllis and Harold NOT to marry. But in truth, his explanation speaks only to the decline in marriage among persons eligible for welfare, i.e., poor pregnant women. It says nothing about the behavior of middle-class women or nonpregnant women who in fact experienced an even greater decline in marriage during the late sixties and early seventies than the women described by Murray, even though they had nothing to gain from the expanding welfare programs.

A satisfactory explanation for the decline in marriage must deal with the broader phenomenon. While there is neither time nor space to explore the topic here, I believe (and others have argued), that the general

decline in marriage, as well as the growth of female-headed families, is a response to broader social changes that began well before the dramatic rise in welfare benefits during the sixties. In particular, I would argue that the trends in family structure are primarily a response to improvements in the employment opportunities of women relative to men, and especially black men.[2]

A final point. Although I find Murray's story about Phyllis and Harold unconvincing, I am not implying that welfare has no effect on family behavior. All else being equal, it is hard to imagine that the availability of welfare does not allow some single mothers to raise their children alone who, in the absence of welfare, might have remained married or given their children up for adoption. In his analysis of welfare effects, however, Murray ignores the variation in the ways in which welfare operates. He also ignores the numerous studies that have attempted to quantify and compare the impact of welfare programs on various dimensions of family behavior. Consequently, his conclusions greatly exaggerate the potential impact of a reduction in welfare benefits.

Welfare may affect the formation of female-headed families in four different ways. It may increase the incidence of illegitimate births, as Murray argues in Chapter 9; it may increase marital disruption, which is the primary cause of female headship among whites; it may reduce remarriage and thereby lengthen the time spent in a female-headed family; and it may encourage single mothers to live independently by providing additional income or in some cases a housing allowance for female heads. Researchers who have examined each of these behaviors find, not

surprisingly, that the welfare effect is strongest for the least consequential behaviors.[3] Most studies indicate that welfare benefits have their strongest effect on living arrangements (single mothers are more likely to be female heads of households in high-benefit states than in low-benefit states), they have a moderate effect on remarriage (high-benefit states have lower remarriage rates than low-benefit states), and they have a weak to nonexistent effect on illegitimate birth rates and divorce/separation rates. This suggests that getting rid of welfare will neither "drastically reduce births to single teenage girls" nor "reverse the trendline in the breakup of families." Moreover, since many single-mother families would have been poor even if the mother had remained married or, in the case of never-married mothers, had married the child's father, it is unlikely that eliminating welfare would increase the socioeconomic mobility of "millions of persons," as Murray would have us believe.

Notes

[1]Reynolds Farley and Albert I. Hermalin, "Family Stability: A Comparison of Trends Between Blacks and Whites," American Sociological Review, 36 (February 1971), 1-17.

[2]William Julius Wilson and Kathryn M. Neckerman, "Poverty and Family Structure: The Widening Gap between Evidence and Public Policy Issues," Conference on Poverty and Policy: Retrospect and Prospects, Williamsburg, Va., December 6-8, 1984; and Irwin Garfinkel and Sara McLanahan, Single-Mother Families and Public Policy: A New American Dilemma? (Washington, D.C.: Urban Institute, forthcoming).

[3]David T. Ellwood and Mary Jo Bane, "The Impact of AFDC on Family Structure and Living Arrangements." Report prepared for the U.S. Department of Health and Human Services. Mimeo., Harvard University, 1984.

Glen Cain: Comments on Murray's Analysis of the Impact of the War on Poverty on the Labor Market Behavior of the Poor

Charles Murray has an adverse view of the War on Poverty and Great Society programs. His discussion of the impact of these programs on the labor market involves three main empirical points, two nonempirical conjectures, and an underlying economic model of behavior. In my evaluation of Murray's analysis, each of these empirical, conjectural, and theoretical points will be raised, followed by a refutation or at least an expression of important reservations.

I. THREE EMPIRICAL ISSUES

Did Poverty Increase During Economic Growth or During Economic Stagnation?

Murray's overall verdict of failure for the War on Poverty follows from his contention that poverty increased in the 1970s despite increased government expenditures to combat poverty and despite, according to Murray, a record of economic growth from 1970 to 1979 that exceeded the record in the 1950s (pp. 58-59). I do not see how anyone could be persuaded by Murray's contention and evidence.

The incidence of poverty in the United States, which declined during the 1950s, fell more sharply in the 1960s when the Great Society programs were initiated, and continued to decrease until 1973. An increase in poverty, as measured by money incomes of families, began after 1973, but the measure of poverty that allows for income from in-kind transfer

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Acknowledgment: I am grateful to Ross Finnie for research assistance.

payments, like food stamps, shows almost no increase throughout the 1970s. The decline in poverty, however, virtually stopped in 1974.[1] The year of the turning point, 1973, is important, as explained next.

The year 1973 was the beginning of a rather steady economy-wide decline in real earnings and family income, as measured by white median incomes. (See Table 1 and Figure 1.) As revealed by Table 1, real median incomes of white and black workers rose at an annual rate of 2.4 percent and 3.0 percent, respectively, from 1948 to 1973, and the white median income declined in only 6 of these 25 years. From 1973 to 1982 real median incomes for white and black workers declined. The annual rates of change were -2.4 percent and -2.2 percent, respectively, and the white median income declined in 6 of these 9 years.

Figure 1 shows a similar picture for family incomes, as measured by the real median income for whites. The trend was more or less steadily up from 1950 to 1973 and steadily down from 1973 to 1982.

The trends in earnings and incomes of workers and families are critically important, because poverty is a household or family concept. Median household income and earnings are logically and historically the principal correlates of poverty, and their trends in the post-1973 period refute Murray's claim that the economy was prospering in the 1970s. We should recognize that the correlation between median family income and the incidence of poverty among persons and families is not because there is a mechanical arithmetic relation between the median and the poverty levels of income. The median is well above the poverty line--at least twice its level--and the median is a statistic that is unaffected by extreme values.[2] The slower rate of decline in poverty in the post-1973

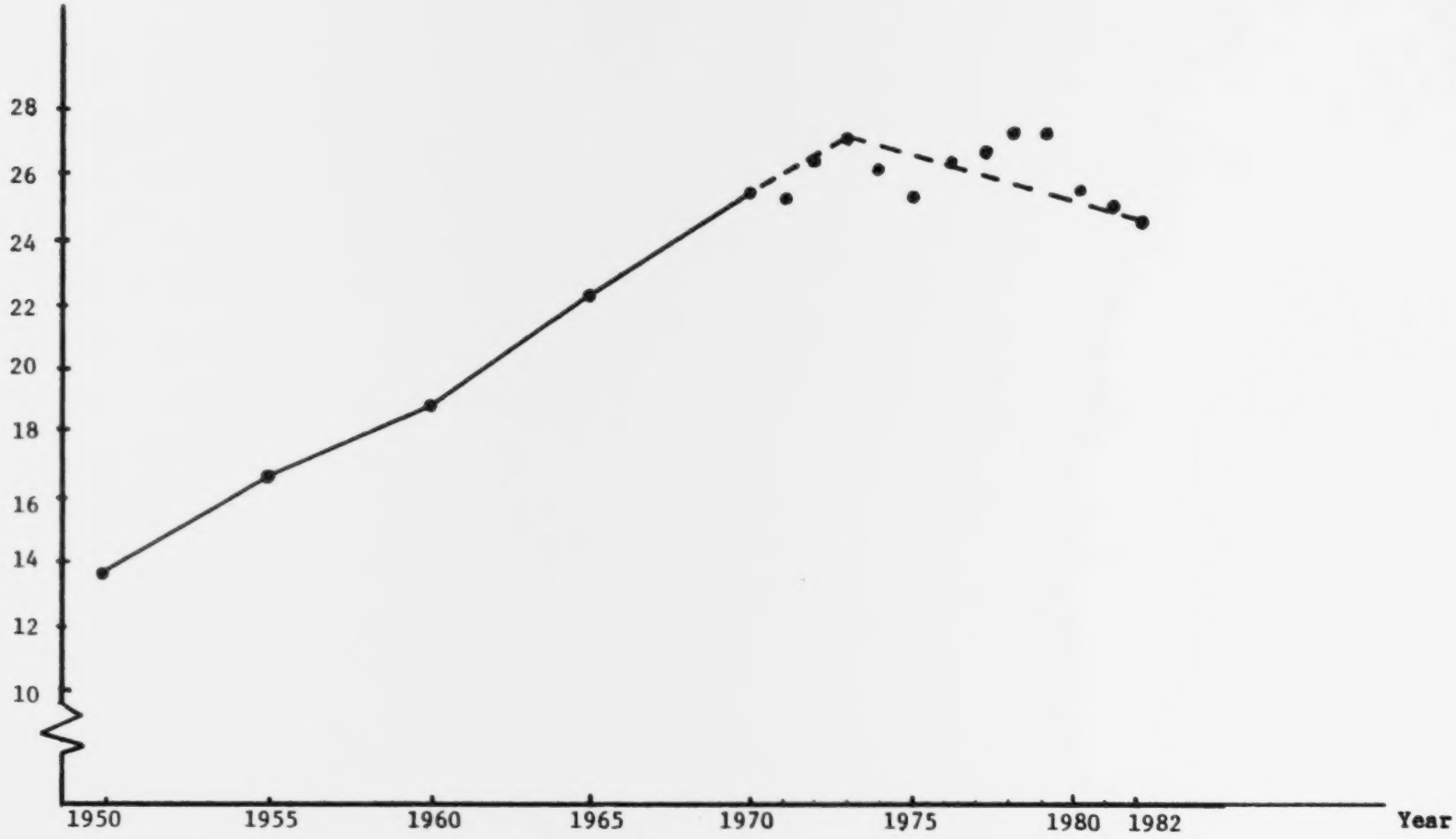
Table 1

Median Money Incomes and Income Ratios for Black and White Male Workers, 1948-1982, in Constant 1982 Dollars

Year	Median Income		Ratio B/W	Year	Median Income		Ratio B/W
	Whites	Blacks			Whites	Blacks	
1948	$10,064	$5,465	.54	1965	$16,185	$ 8,710	.54
1949	10,006	4,844	.48	1966	16,631	9,212	.55
1950	10,862	5,899	.54	1967	16,901	9,653	.57
1951	11,524	6,346	.55	1968	17,388	10,551	.61
1952	11,837	6,487	.55	1969	17,812	10,508	.59
1953	12,237	6,760	.55	1970	17,428	10,490	.60
1954	12,080	6,011	.50	1971	17,248	10,351	.60
1955	12,776	6,724	.53	1972	18,029	11,100	.62
1956	13,558	7,113	.52	1973	18,360	11,551	.63
1957	13,402	7,096	.53	1974	17,330	11,135	.64
1958	13,275	6,614	.50	1975	16,679	10,511	.63
1959	13,937	6,561	.47	1976	16,849	10,540	.63
1960	14,003	7,367	.53	1977	16,889	10,326	.61
1961	14,290	7,385	.52	1978	16,945	10,796	.64
1962	14,859	7,318	.49	1979	16,363	10,604	.65
1963	15,151	7,874	.52	1980	15,612	9,786	.63
1964	15,361	8,708	.57	1981	15,172	9,624	.63
				1982	14,748	9,493	.64

Source: U.S. Bureau of the Census, Current Population Reports, Series P-60, No. 142, "Money Income of Households, Families, and Persons in the United States: 1982" (Washington, D.C.: GPO, 1984), Table 40.

Figure 1
Median Income in Constant (1982) Dollars for White Families, 1950-1982 (Selected Years)



Source: Statistical Abstract of the United States: 1984 (104th Edition) (Washington, D.C.: GPO, 1983) p. 463.

Note: 1950-1970: End points for 5-year intervals are connected.
1970-1982: Actual Scattered diagram, plus the end points for 1970-1973 and 1973-1982 are connected by dashed lines.

period is surely related to the same economy-wide forces that pulled down the average level of earnings for all workers during these years.

Murray based his claim of economic prosperity in the 1970s on the trend in per capita gross national product, referring to its higher rate of growth in the 1970s than in the 1950s. However, GNP per capita is only indirectly related to poverty statistics. GNP involves some technical components like depreciation and capital consumption, which have no direct relation to poverty. More important, GNP per capita can be a misleading indicator because it is not sensitive to the "dependency ratio," which is the ratio of the non-working-age population to the total population; whereas both GNP and poverty are sensitive to this ratio. An illustration of how an increase in per capita income may accompany a decrease in family income and an increase in poverty is provided in the hypothetical example for two periods in Table 2.

We see that in period 2 per capita income has risen from $3,000 to $3,167, but the incidence of poverty has also risen from zero to 100 percent. The illustration is extreme, of course, but its realism with respect to the comparison of dependency ratios for the 1950s and the 1970s is apt. In 1960 the population aged 15 and younger was 33 percent of the total population; in 1980, this percentage was 24.[3]

The figures for median income and earnings measure central tendencies in two important indicators of the performance of the economy that are relevant to poverty, but they do not reveal how this performance is distributed, particularly as regards the poorest 20 percent or so of the population. One of the most important distributional indicators is the unemployment rate, and this rose substantially from the 1950s and 1960s

Table 2

Illustrating How a Decrease in the Dependency Ratio Can Increase per Capita Income, Decrease Family (or Household) Income, and Increase Poverty

Assume the economy has six persons in time periods 1 and 2.

Demographic Unit	Family or Household Income	Poverty Level (By Size of Household)[a]
Period 1: (A high-fertility population.)		
One family: 2 adults, 3 children	$12,000	$11,884
One household of 1 adult	$6,000	$5,019
Per capita income = $3,000 (= $18,000/6)		
Dependency ratio = 50 percent (= 3 children/6-person population)		
Incidence of poverty = 0 percent		
Period 2: (Twenty years later: It is assumed that marriage rates, birthrates, and wages for working adults have all declined.)		
One family: 2 adults, 2 children	$9,000	$9,862
Two one-person households of adults	$5,000 $5,000	$5,019 $5,019
Per capita income = $3,167 (= $19,000/6)		
Dependency ratio = 33 percent (= 2 children/6-person population)		
Incidence of poverty = 100 percent		

Note: Per capita income rose in period 2, yet every household has a lower income, which is now below the poverty line, and each family member in the multiple-person household has a lower income.

[a]These are the poverty-level incomes in 1982 for households of sizes 5, 4, and 1. (See Statistical Abstract of the United States, 1984, p. 447.)

to the 1970s and 1980s.[4] It is reasonable to believe that the rises in unemployment rates in the 1970s and 1980s contributed to the rise in poverty over and above their role in diminishing median levels of earnings and incomes.

I conclude that the first empirical point by Murray, namely, that poverty increased in the 1970s despite an economic boom, is only half right. Poverty increased, but there was no economic boom. Instead, there was an economic decline, and this is an alternative explanation to the one proposed by Murray.

Blacks in the Labor Market: What Gains? What Losses?

Murray's argument that the War on Poverty failed in the labor market is almost entirely based on the decline in the labor market status of black teenagers. Their unemployment rates rose and their labor force participation rates (LFPRs) fell, despite increases in government expenditures directed toward this group. On two parts of Murray's argument, I am in full agreement. First, the economic and labor market status of black teenagers, especially those living in the central cities, is a major social problem. Second, our concern is not simply that their labor market status worsened as a consequence of the general economic slowdown, which is part of the problem, but that their status worsened relative to that of white teenagers.

Although I agree with these points, I will argue below that Murray's conclusion that the War on Poverty failed overall in the labor market for blacks is not convincing, and that Murray exaggerates the failure regarding black teenagers.

By Murray's own admission (pp. 85-92), the overall comparison between black and white workers, which involves many more people than the comparison of teenagers, shows a number of important gains by blacks, both relative to whites and in absolute terms. As shown in Tables 1 and 3, the ratio of black earnings to white earnings among men rose in the latter part of the 1960s and, generally speaking, throughout the 1970s. (See column 4 in Table 3 for a summary that spans four decades.) These ratios are less than one and reveal considerable inequality, but the years since the War on Poverty began in 1964 show an upward trend. Morever, the ratio of black earnings to white earnings among women rose even more rapidly during the past 20 years, and black women's median earnings in the 1980s for year-round full-time workers is nearly equal to that for white women. (See column 5 in Table 3.) Murray also cites evidence for relative and absolute gains among black workers in occupational attainment and in the economic returns to education.[5] All these developments are of major importance, yet Murray dwells on the case of black teenagers to advance his overall negative verdict of the antipoverty programs of the Johnson-Nixon-Ford-Carter administrations. His case would be distorted even if he were accurately describing the status of black teenagers, and I will turn next to the distortions in this part of his attack.

Murray fails to give credit to some of the government programs that were aimed at black (and white) teenagers for doing what they intended to do, which was to increase school enrollments; specifically, to reduce dropping out of high school and to raise college attendance. By this objective, the programs had considerable success. School enrollments

Table 3

Median-Earnings Ratios for Year-Round, Full-Time Workers, Gender and Ethnicity Comparisons, Annual Averages for Four Periods, 1939-1982

Year or Period[a]	Women/Men Earnings Ratio by Ethnicity			Black/White Earnings Ratio		Hispanic/White Earnings Ratio	
	White (1)	Black (2)	Hispanic (3)	Men (4)	Women (5)	Men (6)	Women (7)
1939[b]	.61	.51	—	.45	.38	—	—
1955-1966[b]	.61	.61	—	.62	.65	—	—
1967-1974[c]	.58	.70	—	.68	.83	—	—
1975-1982[d]	.59	.76	.70	.73	.94	.72	.86

Sources: U.S. Bureau of the Census, Current Population Reports, Series P-60, Nos. 43, 137, and 140. The full citation for No. 137 includes the title: "Money Income of Households, Families and Persons in the United States, 1981" (Washington D.C.: GPO, 1983).

[a]The years 1955-1982 are divided into three periods, and the average of the annual ratios are reported for each period. The first year for the continuous time series of earnings for year-round, full-time workers is 1955, but the 1940 census provides this figure for 1939.

[b]Ratios are for wage and salary earnings (excludes self-employed workers) for whites and nonwhites, who are defined as blacks and other nonwhite races in later Census publications.

[c]Ratios are for all earnings (includes self-employed workers and self-employment income) for whites and blacks. The first year for which blacks are reported separately is 1967. The black/white ratios are, on average, about .01 lower than the nonwhite/white ratios for men, and about .02 lower for women. The trends in both ratios, black/white and nonwhite/white, are virtually identical.

[d]Same as c; also, 1975 is the first year in which earnings are reported separately for Hispanic workers.

rose from, say, 1960 to 1980, for both white and black youth, and blacks gained relative to whites. Murray shows these absolute and relative gains in school enrollment among blacks for two relevant age groups, 14-17 and 20-24 (p. 252), but he does not inform the reader that some labor market programs of the War on Poverty, principally the Neighborhood Youth Corps, were aimed at this goal.

Now consider that an increase in school enrollment generally lowers the LFPRs of young people, and it may raise their unemployment rates; two results that call for a reexamination of Murray's sweeping negative judgment about the change in the status of black teenagers. The general point is that a low LFPR for teenagers need not be considered a sign of failure if the teenagers who are not holding jobs are full-time students. In Table 4 I show a hypothetical numerical example to help explain how a program that is successful in terms of increasing school enrollment may serve to lower the LFPR and raise the unemployment rate.

Table 4

Illustrating How an Increase in School Enrollment Can Increase Unemployment Rates (UR) and Decrease Labor Force Participation Rates (LFPR)

Enrollment and school labor force status of 100 boys, aged 16-17.

Period 1

40	in school and not in the labor force (NLF)
40	at work and not in school (high school dropouts who have jobs)
10	unemployed--without a job but looking for a job; 5 are in school and 5 are not in school
10	NLF and not in school
100	Total

Enrolled in school: 45 percent
LFPR: 50 percent [= (40 + 10)/100]
UR: 20 percent (= 10/50)

Period 2

Assume a program, like the Neighborhood Youth Corps, has the effect of subsidizing or persuading 10 of the dropouts to return to school as full-time but nonworking students, and assume that these "successes" are selective of those among the dropouts who have had jobs. The new statuses are as follows:

50	in school and NLF
30	at work and not in school
10	unemployed--5 in school; 5 not in school
10	NLF and not in school
100	Total

Enrolled in school: 55 percent
LFPR: 40 percent (= (30 + 10)/100)
UR: 25 percent (= 10/40)

Murray raises and then dismisses the point that rising school enrollment rates among blacks could explain their declining LFPRs. His device is to present the following statistics on the change (decline) in LFPRs for young black men, and the increase in their enrollment proportions for the 1965-1970 period (pp. 78-79).

Murray's Comparison of Changes in LFPRs and School Enrollments: 1965-1970

Age Group	Decrease in LFPRs of Nonwhite Males	Increase in Percentage of Nonwhite Males Enrolled in School
16-17	-4.5	+1.8
18-19	-4.9	+ .5
20-24	-6.3	+5.2

He remarks that the increase in school enrollment "explains at best a small fraction of the reduction in black LFP" (p. 79).

The selection of this particular 5-year period is misleading. The broader span of time during which the Great Society programs and their aftermath are being analyzed by Murray shows a different picture. Consider the following comparison of LFPRs and enrollment proportions for the period 1960-1979.[6]

An Alternative Comparison of Changes in LFPRs and School Enrollments: 1960-1979

Age Group	Decrease in LFPRs of Nonwhite Males	Increase in Percentage of Nonwhite Males Enrolled in School
16-17	-13.9	+15.5
18-19	-13.4	+11.1
20-24	-10.3	+10.4

These figures imply that the increases in school enrollment are a major source of the decline in LFPRs of black teenagers. We do not have to share Murray's belief that the decline in LFPRs is largely a reflection of the decline in the work ethic among black teenagers.

There is a further source of distortion in Murray's use of the gap in the white/black LFPRs among male youth to demonstrate the "failures" of the Great Society programs and the demise of work values among blacks. I refer to Murray's use of civilian labor force statistics despite the facts that military service (a) is an important source of employment for young men; (b) is recognized as being part of the official labor force today; and (c) is more prevalent among black youth today than among whites--in contrast to the period before 1972 when a higher proportion of whites were in the military. Using the figures made available by Mare and Winship[7] for 1964 to 1978, we may construct another set of statistics, like those above, to show that the difference in military service also presents young black men in a more favorable comparison with young white men than Murray portrays.

Various "Activity" Rates for White and Nonwhite Young Men, 1964-1978

	(1) Change in Civilian LFPR's		(2) Change in Proportions in the Military		(3) Change in Civilian and Military LFPRs		(4) Change in Proportions Enrolled in School	
Age Group	White	Nonwhite	White	Nonwhite	White	Nonwhite	White	Nonwhite
18-19	+8.7	- 7.7	- 5.8	+1.5	+2.9	- 6.2	-5.3	+10.6
20-24	+1.5	-11.4	-10.7	-0.1	-9.2	-11.5	-1.0	+11.1

As shown above, the large black/white gap of 13 to 16 percentage points in civilian LFPRs, which Murray has emphasized, is sharply

reduced to 2 to 9 percentage points in total (civilian plus military) LFPRs. (Compare the columns under (1) to the columns under (3).) An additional allowance for the relative gains in black school enrollment further moderates Murray's alarmist conclusions.[8] (See the columns under (4).)

Mare and Winship offer another measure of the comparative status of young white and black males that includes three components of "productive activity:" the employed (but excluding the unemployed), those enrolled in school (who have not already been counted among the employed), and those in the military. Over the span of years reported by Mare and Winship, 1964 to 1978, the rates of "productive activity" are as follows:

	Proportion of Male Youth Who are Productively Active,* by Race					
	1964		1978		'64-'78 Change	
Age Group	White	Nonwhite	White	Nonwhite	White	Nonwhite
16-17	96.7	91.6	96.4	96.3	-0.3	+4.7
18-19	92.0	85.4	95.3	86.8	+3.3	+1.4
20-24	93.9	89.5	94.1	84.1	+0.2	-5.4

*Number per 100 who are either employed, in school, or in the military service (but without double-counting any of the three forms of productive activity).

Again, the relative status of young black men is not nearly as grim as Murray claims, which is not to deny that the labor market statuses of black youth are seriously lagging behind those of white youth.

The major source of the higher LFPRs among white youth is that white teenagers who are in school have increased their LFPRs, while black teenagers have not. Murray's "supply-side" explanation is that whites

exhibit a work ethic and blacks do not. But what about the demand side? I conjecture that the major source of jobs for white teenagers in recent years has been the mushrooming growth of shopping centers, supermarkets, and fast-food restaurants in white communities, especially in suburban areas. Black teenagers confront quite different demand conditions because they live in central cities, where stores are being shut down. Even after reading Murray's book, I am willing to suggest that the disadvantaged employment position of black teenagers is "not their fault." (Those who have read Losing Ground will recognize the quoted phrase as Murray's bogeyman—a phrase expressing an attitude that Murray claims to be driving black people, or maybe just young blacks, to ruin.)

Interpreting the Experiments on the Negative Income Tax

Murray's third empirical point involves his interpretation of the results of the negative income tax experiments. He says "the results among husbands were disappointing" to the investigators (p. 151); that "the NIT had a disastrous impact" on the hours of work of "young males who were not yet heads of families" (p. 151); and that the "NIT experiment made a shambles of the expectations of its sponsors," advocates, and researchers (p. 153). I confess that I took part in the research on these experiments, and I further confess that I do not recognize the motivations for or the results and interpretations of these experiments as Murray describes them.

Murray's statement that the motivation of the experiments was to establish "proof . . . that a guaranteed income would not cause people to reduce their work effort . . ." (p. 149) is false. (Italics in original.) The experiments, let us recall, were aimed, not at people on

welfare to see if they would work more, but at working families which were not on welfare to see how much less members of these families would work. The investigators expected to find reductions in work in response to receiving the income guarantees. Those who want to learn about the motivations behind the negative income tax experiment should read Albert Rees's brief introductory chapter to the volume reporting on the New Jersey negative income tax experiment.[9] Contrary to Murray's statements (p. 149 and footnote 4, p. 288), the experiments adopted income maintenance plans that were very similar to those advocated by the conservative economist Milton Friedman.

Murray says that the advocates and researchers of the NIT programs were left in a "shambles" because the reductions in work reported in the research on the experiments were so large. To refute this claim I need only discuss the main experimental results from the New Jersey and Seattle/Denver experiments.

The New Jersey (NJ) experiment found reductions in work by husbands that were less than 5 percent: from about 34 hours of work per week by the control group to about 32 hours by the treatment group in the "medium and high" plans. For wives, the reduction was about 25 percent, but it was pointed out (see below) that this was on a very low base and did not represent an important income loss.[10]

In the Seattle-Denver Income Maintenance Experiment (SD), husbands and wives reduced their work by 9 and 20 percent, respectively, using the figures Murray cites (p. 151). Murray does not tell the reader, however, that the SD experiment provided much more generous plans than NJ; indeed,

more generous than any welfare-reform plan contemplated. In fact, the NJ plans, as well as the SD plans were, on average, more generous than any plans considered by Congress or the executive branch of government.[11]

Now, there are good reasons why some of the experimental plans should be generous even if they are not politically feasible. Generosity in the experimental programs implies that the parameters of the experimental "treatments," such as their guarantee levels, will vary widely and vary on the side of being more generous. You cannot, after all, run a social experiment and make people worse off than they would be without the experiment, so "wide variation" must mean wide variation on the high side. Given that the purpose of experiments was to measure the behavioral responses to variation in these treatment parameters, designing their variation to be wide is a sound statistical principle. Of course, some of the variation in the treatments must involve politically feasible ranges, and all of the experiments were designed to achieve this feature as well. However the observed differences between treatment and control families in the experiment should not be assumed to measure what would occur in the much "cheaper" plans that are advocated by public officials.

Let us next examine why Murray regards the work reductions found in the SD experiment to be "disastrous" in their own context. Murray says that if the 9 percent reduction by husbands were spread across all husbands, this would not be so bad, but if the reduction is the result of 9 percent of the husbands dropping out of the labor force, this is bad (p. 151). A contrary view is more reasonable. It is likely that the husbands who drop out are those with the lowest earnings abilities and who are only marginally attached to the labor force. It is likely, there-

fore, that society loses less by their "early retirements" than if the same reduction in hours of work were evenly distributed across all husbands.

Murray views the 20 percent reduction in work by wives with alarm because these wives were "precisely the ones who were in a position to cause the most long-term damage to the goal of reducing poverty," and he speaks of wives who would "push a family out of the poverty trap and into a more secure long-term future" (p. 151). A closer look at the data dispels Murray's claims.

In the NJ experiment the wives were in poor families and had very low earnings on average. Clearly, if a wife had been working and earning much, then her earnings, combined with her husband's earnings, would have tended to raise the family above the poverty line. To be specific, the average annual hours of work for control-group wives in NJ was about 250 per year.[12] This was about 25 percent more than the experimentals, but the base is so low that the experimental wives could have matched the controls' amount with just 63 more hours of work per year--hardly a route to long-run security.

The SD experiment included many families with incomes above the poverty line, and, therefore, the average time spent in market work by wives in these families was greater--about 600 hours per year by control-group wives.[13] Set aside the fact that these relatively higher income families would not be eligible for government income maintenance plans. Let us assume that they would be eligible and that they were covered by a plan as generous as that in the SD experiment. Assume that these wives would reduce their work by 20 percent. The average reduction is, there-

fore, 120 hours per year and at an average wage of say $5 per hour, their families' earnings loss would be $600 per year. Again, losses of this magnitude, amounting to about 5 percent of the poverty-line income today for a family of four, could hardly make a major difference in the poverty status of these families. Nor is it likely that the families that are covered by a negative income tax plan, and in which the wives reduce their work, are worse off. The family's total income has increased as a result of income transfer payments that exceed the loss in earnings, and the alternative uses of the wife's time in the home sector may improve the well-being of the family. Murray's judgment that the experimental results were disastrous is unsupportable.

My final comment about Murray's evaluation of the experiments is methodological. Murray is incorrect in his claim (pp. 152-153) that all the "biases" in the experimental analysis serve to understate the work disincentives. Here are three that serve to overstate the measured work reductions: (a) attrition bias, because families that work more and rise above the "breakeven" level of income will receive no payments and will, therefore, be more likely to attrite than families that work less and receive payments; (b) underreporting of income is likely to be greater by families that are in the treatment group and who will receive larger payments if they report lower incomes;[14] (c) the intertemporal shift of leisure consumption to the experimental period, which the participants knew to be temporary.[15] Finally, Murray is incorrect when he states (p. 153) that the work reductions were larger for the subgroup in the SD experiment that was guaranteed income for a 20-year period. No work reductions were found for this group.[16]

II. MURRAY'S CONJECTURES

According to Murray, the Great Society programs of the 1960s marked an abrupt change in the way Americans viewed the problem of poverty and harmed not only society but the poor themselves--especially blacks. He offers two conjectures in support of these two theses. The first is that the intellectual elite in America imposed on society a philosophical doctrine that poverty was no longer the fault of the individual but rather of the "system." In Chapter 3 Murray refers to this shift as analogous to the change from an Aristotelian to a Newtonian view of the universe, and 1964-1967 are his watershed dates for this shift in philosophy.

To the extent that any evidence for this thesis may be said to be offered, Murray refers to the growth of federal programs after 1964 that tax the nonpoor and give money and other forms of assistance to the poor and to actions of the government, including court decisions, that intervene in the economy on behalf of the poor and blacks.

Murray's sweeping judgments cannot be evaluated briefly, if, indeed, they can be evaluated satisfactorily at all, but with respect to the growth of federal programs and intervention, I doubt that the three decades before the 1960s provide evidence for an abrupt change. Surely the period of the 1930s and its economic depression and New Deal reforms mark the most dramatic shift in the role of government in providing for the economic welfare of the individual. The decade of the 1940s saw a wartime expansion of the government and its role in the economy that carried over into the postwar period as well. The 1950s, although often referred to as a period of retrenchment in the role of government, was in fact the

decade for the most sweeping expansions in the Old Age, Survivors, and Disability Insurance program (OASDI), then, as now, the largest income transfer program. (The expenditures of this program grew more rapidly in later years, but the expansion in entitlements in OASDI occurred in the 1950s.) In light of this 30-year evolution, Murray's thesis of an abrupt change in values about personal and societal responsibilities does not seem persuasive.

A second conjecture by Murray is that government aid to the poor during the 1960s and afterwards went to the "undeserving poor"--those who did not want to work or learn--and imposed costs on the "deserving poor," with, again, special reference by Murray to blacks. (See Chapter 14 and pp. 200-201.) Murray provides no evidence for this conjecture. Indeed, anyone familiar with the evaluation of many of the War on Poverty programs knows that the opposite conjecture was more commonly claimed; namely, that the public employment, training, and education programs tended to "cream" the most able among the eligible population.

III. MURRAY'S ECONOMIC MODEL OF THE BEHAVIOR OF THE POOR

Murray's explanation for the behavior of recipients of aid from government programs appears to be derived from a conventional economic model in which the poor act as rational consumers. They attempt to improve their consumption of goods and leisure in response to the incentives and disincentives of the programs directed at them. Most economists would accept this general framework, which is why, for example, economists expected that a higher income guarantee combined with higher

taxes on earnings would discourage work by the recipients of a negative income tax.

If one adheres to the view that the individual generally is the best judge of his or her own well-being, then programs offering benefits to the poor should improve their well-being. After all, the poor can voluntarily choose whether or not to participate in the programs.

At this point, however, Murray departs from the conventional economic model, because he claims that the poor are made worse off as a result of the provision of, in particular, transfer-payment programs. It turns out that for Murray the rationality of the poor extends only to the fleeting short run. In the relevant long run the poor, especially the black poor, behave irrationally when they choose to avail themselves of program benefits. In Murray's words, they strike a "Faustian bargain" (p. 176). They hurt themselves. (See pp. 176-177 and Chapter 17.)

Murray's model of this self-destructive behavior has no basis in economics, which is not to say that the model is incorrect. But his model should be recognized for what it is: his own set of assumptions about what is good and bad for the poor. I conclude that Murray's theory of behavior, like his conjecture about changing values in society and like his empirical evidence, does not support a verdict of failure of the Great Society programs.

Notes

[1]The official measure of the incidence of poverty shows a relatively steady decline from 22.4 percent of the population in 1959 to 11.1 percent in 1973, at which time it begins to rise, and it reaches 13.0 percent in 1980 and 15.0 percent in 1982. (See Statistical Abstract of the United States, 1984, 104th Edition [Washington, D.C.: GPO, 1983], p. 471.) Murray acknowledges (p. 63) that in-kind transfers to the poor grew substantially in the late 1960s and that a measure of poverty that allows for these shows no increase in the incidence of poverty until some time after 1973. For statistics on this measure of poverty, see the citations to Timothy M. Smeeding in Murray, footnote 8, p. 273 and Figure 4.5, p. 65.

[2]In 1975 and 1980, for example, the poverty levels of income for a family size of four were $5,500 and $8,414 in current dollars, whereas the median family incomes for whites in those years were $10,236 and $21,904, respectively (Statistical Abstract of the United States, 1984, pp. 447 and 463).

[3]Statistical Abstract of the United States, 1984, p. 31.

[4]The average annual rate of unemployment from 1974 to 1981 was 6.9 percent, and from 1974 to 1983 it was 7.5 percent. These average rates are about 50 percent higher than the averages of 4.6 percent for the previous 10 years, 1964-1973, and 4.8 percent for 1947-1963. If we restrict our comparison to unemployment rates for a group that is one of the least likely to be part of the poverty population--say, white males between 35

and 54 years old—the conclusion is still that of a substantial proportionate rise from, say, 1964-1973 to 1974-1982. The average unemployment rate for this demographic group more than doubled, increasing from 1.7 percent in the early period to 3.5 percent in the latter period.

[5]The term "returns to education" refers to the effect of years of schooling on the labor market earnings of persons during their post-schooling working careers. See Murray, pp. 90-91.

[6]The sources for these statistics on black males are U.S. Bureau of the Census, Current Population Reports, Series P-20, No. 360, "School Enrollment: Social and Economic Characteristics of Students," October 1979 (Washington, D.C.: GPO, 1981) p.61; same sources, same title, No. 260, February, 1974, p. 61; and U.S. Department of Labor, Employment and Training Report of the President, 1981 (Washington, D.C.: GPO, 1981), p. 128. The reader should be warned of a substantial volatility in the statistics on enrollment proportions and LFPRs for black male teenagers. Sampling variability and sensitivity to the business cycle are two likely sources of this volatility.

[7]Robert D. Mare and Christopher Winship, "Changes in Race Differentials in Youth Unemployment and Labor Force Participation, 1950-1978: Preliminary Analysis," in Expanding Employment Opportunities for Disadvantaged Youth, Special Report No. 37, National Commission for Employment Policy (Washington, D.C.: GPO, December 1979) p. 41. The changes in the civilian LFPR and in the school enrollment proportions are taken from the sources cited in footnote 6.

[8]The increase from 1964 to 1978 in the proportion of black males aged 18-19 who are enrolled in school does appear to overstate the trend in this period, because the 1964 enrollment proportion was unusually low. If the average enrollment rate for the three years, 1963-1965, is used as the base-year proportion, the increase by 1978 is only 5.9 percentage points instead of 10.6. Nevertheless, an increase of 5.9 is in sharp contrast to the decrease of 5.3 for white males aged 18-19.

[9]Harold W. Watts and Albert Rees, eds., The New Jersey Income Maintenance Experiment: Vol. II, Labor-Supply Responses, Institute for Research on Poverty (New York: Academic Press, 1976) pp. 5-32.

[10]See Watts and Rees, pp. 22-28. Note that the "medium and high" plans refer to the plans that were relatively generous in terms of providing higher income guarantees and in having lower benefit-reduction rates. (A benefit-reduction rate refers to the percentage by which the recipient's transfer payment is reduced for each dollar increase in the recipient's earnings.)

[11]Consider that the Family Assistance Plan (FAP), proposed by the Nixon administration in 1969, offered an income guarantee of 45 percent of the poverty level and a benefit-reduction rate that was approximately 50 percent. Including several noncash transfers, the income guarantee associated with FAP was 79 percent of the poverty line and the benefit-reduction rate was approximately 70 percent. (See D. L. Bawden, G. G. Cain, and L. J. Hausman, "The Family Assistance Plan: An Analysis and Evaluation," Public Policy, 19 (Spring 1971), 323-351. These parameters, translated into income figures for 1982, when the poverty line for a family of four was $9,862, imply that the income guarantees for a

family of four would have been $4,438 (cash) or $7,791 (cash plus noncash) and that the breakeven levels of income (above which no benefits are received) would have been around $9,000 (for the cash program) or $11,130 (for the cash-plus-noncash program). In contrast to these indicators of the generosity of the program, consider that the average plan in the SD experiment had an income guarantee of 110 percent of the poverty line and a benefit-reduction rate of 50 percent. Thus, a family of four in 1982 would be guaranteed $10,848, and its breakeven level of income would be $21,698. An allowance for medical insurance benefits (or other forms of noncash benefits) would raise both these figures. It is no exaggeration to say that the average income maintenance plan in the SD experiment is twice as generous as the sort of plan proposed, but never enacted, during the times when these experiments were conducted. Since that time, no negative income tax plan has been proposed by any administration.

[12]Watts and Rees, p. 134.

[13]The statistics about the SD experiment are from SRI International, Final Report of the Seattle-Denver Income Maintenance Experiment, Vol. 1, Design and Results (Menlo Park, Calif.: SRI, May 1983).

[14]For evidence of this bias and of the difference it may make, see David Greenberg and Harlan Halsey, "Systematic Misreporting and Experimental Effects on Work Effort: Evidence from the Seattle-Denver Income Maintenance Experiment," Journal of Labor Economics, 3 (October 1983), 380-407.

[15]This is the one bias in the "other" direction that Murray indirectly acknowledges with a citation to a study by Charles Metcalf. See Murray (pp. 288-289, footnote 21).

[16]SRI International, p. 125.

Michael Olneck: Comments on Schooling

Alice Rivlin, in reviewing Christopher Jencks's book Inequality a dozen years ago,[1] coined the term "forensic social science," which she said was coming into vogue in the late sixties and early seventies. That was a style in which social science evidence was marshalled in support of policy positions and arguments. The rules of the game were that you presented your case as convincingly as possible and left it to somebody else to give the other side. Dr. Murray has indicated in his earlier comments that he would hate for his book to be taken for a tract that plays footloose with the data. Nevertheless, to a naive reader, Losing Ground does read like an argument from presuppositions for a set of policies and approaches, bolstered by data that add up, convincingly, at least in the mind of the author. I'm going to suggest that the data don't add up quite the way Dr. Murray claims they do, and that, therefore, the policy analyses advanced in Losing Ground are seriously flawed.

The empirical premise in Murray's account of education is that the disadvantaged, with blacks as a proxy, were making appreciable strides in educational achievement and education attainment during the 1950s and early 1960s, but that subsequent to 1965, approximately, when federal initiatives in education reform grew, that this process was blunted, and even reversed, such that the gap in educational achievement between black and white students leaving high school was so great that it threatened to defeat any other attempts to narrow the economic differences separating blacks from whites. Clearly, a story of deterioration and decline. The explanation deduced for this turn of events is that the federal interventions in education were of a piece with policy in other areas, in that

they entailed perverse incentives, that, in the end, promoted self-destructive behavior and failure.

The solutions Murray proposes for the failure of the schools to educate the disadvantaged are (1) free tuition through graduate school; (2) vouchers for the poor; and (3) (a very intriguing idea, I must say), entrance exams for all subjects in courses at all grade levels starting with grade one (pp. 223-227). Also, in general, Murray recommends a "return" to stress on distinctions and status as a reward for effort and accomplishment.

Now since our receptiveness to Murray's explanatory framework and to his policy prescriptions depends very much on the validity of his central empirical proposition, i.e., that we did lose ground after 1965, I am going to concentrate my attention on that issue.

Did blacks lose ground in education to whites subsequent to 1965, after having enjoyed a period of progress? I do not think that Murray's own data taken alone support that case. Nor, taken together with additional evidence, do they support the case. Whatever the (very debatable, not to be taken for granted) extent and locus of deterioration in American education during the last 20 years, it seems clear to me, from the kind of evidence that is presented in the book, and the kind I have available to me, that blacks, on average, stood in no worse relation to whites in 1980 than they did in 1965, and may well have made gains. Now, perhaps the quantitative data are an inadequate guide in these matters. I think that the comments about looking directly inside city schools, conducting quasi-ethnographic research, etc., are very important, because it may well be that there are settings in which what is being described

in the book has, in fact, occurred. But, if there are, these settings do not include a sufficient number of students for the changes to have shown up in large-scale data that's nationally representative.

A second thing that might have happened which could support Murray's thesis is what Daniel Moynihan has called the "schism in Black America"[2] many years ago, with respect to diverging class structure within the black community, could well have been occurring in education. If we look at averages, as Murray does, and don't look at such things as percentage of failures, percentage below some threshold, what have you, we may be missing things. Those who are learning best may learn more, while those who are learning little may be learning less, and yet the averages could stay the same. Murray does not, however, present such evidence, and I have not, in preparing for this, taken the time to find and collate it, but it is a possibility to be kept in mind.

Murray's evidence for pre-1965 educational progress is really only two pieces of information, three if we distinguish college attainment from high school attainment. One is that there was racial convergence in the enrollments of high-school-age youths 14-17, to something like the saturation point (over 90 percent), as of the early and mid-1960s.

Second, and it's indirect evidence, cross-sectional results show that the effects of educational attainment on earnings was greater for younger men than for older men, and show a monotonically rising relationship to age in a sample analyzed by Finis Welch in the early 1970s.[3] The argument for the relevance of these results to the issues at hand is that if schooling is becoming increasingly effective, if people are taking an increasing amount of learning and skills away with their attainment, then

we should see increases in the effects of the pecuniary consequences of schooling. Accompanied by converging enrollment rates, these results would indicate that the increasingly similar educational attainment between blacks and whites was occurring at higher levels of achievement.

Now, first we have to observe that because of variations in grade retardation, enrollment rates are an imprecise measure of eventual attainment. This problem is of particular consequence in cross-group comparisons. While pre-1965 gains, correctly inferred by Murray from enrollment rates, are indisputable, actual gains of blacks relative to whites in attainment of a high school degree did not, as Murray contends, end in the mid-1960s. Digest of Education Statistics data show clearly that high school graduation rates converged by race, at least through the high school class of 1978, and that there was a 5 to 6 percent drop in the black drop-out rate for 18- to 21-year-olds between 1971 and 1981.[4] (The discrepancy between the pattern of enrollment rates, reported by Murray, and direct evidence on attainment levels implies that while relative to whites, blacks did not, after 1965, increase their propensity to be in school, those blacks who were enrolled were relatively more likely to graduate.)

I think Murray missed the chance here, however, to strike a blow against some of the optimism of certain of the War on Poverty or collateral reforms, in talking about attainment of college. On that issue, he seems to say "Yes, we gave financial aid and things continued to shoot up after the mid-1960s, and they don't start shifting downward until you get to 1977" (pp. 99-100). Further, he notes that there was an increasing rate of college attendance among both blacks and whites, but the black rate increased faster, so blacks gained ground on whites. I find it very

disturbing to have discovered, in preparing this paper, that for what I calculate to be the high school classes of 1964 to 1978, the college graduation rates of blacks and whites were stable, and there is no convergence: 23 percent for whites, about 16 percent for blacks.[5] I will leave it to Lee Hansen[6] to explain how this is possible in light of the financial aid we gave.

With respect to Murray's second argument, increasing coefficients for education in earnings equations may or may not signify increased educational quality. The data Murray cites pertain to people age 14 to 70, so we have serious selection problems of people not working, owing to retirement or not having entered the labor force. Some of the coefficients Murray reports are so high, e.g., 23 or 28 percent increases in earnings based on one year of schooling, as to suggest that zero earners must have been included in the sample, and that what we are getting is a mix of the effects of schooling on earnings with the effects of schooling on whether you work or do not. If this is the case, it is hazardous to interpret upward trends in the coefficients as indicative of shifts in educational quality. The results may simply reflect the progressively smaller probabilities for older persons (with relatively lower educational attainment) to be in the labor force.

But, accepting Murray's logic here, it is important to note, that using Census data for men 25 to 64, Reynolds Farley, in his recent book Blacks and Whites: Narrowing the Gap? shows appreciable increases between 1969 and 1979--in the coefficients--in earnings equations for both years of elementary or secondary school and years of college among blacks.[7] No similar increases are evident for whites, and no increases

are evident for either race between 1959 and 1969. On this evidence, according to Murray, I must conclude that American education improved not between 1959 and 1969, but between 1969 and 1979. Such a conclusion, of course, flies in the face of assumed declines in standardized test scores. The safer conclusion is that these data tell us little of interest for answering the questions Murray poses.

Turning to the evidence of the post-1965 decline, I think Murray rather uncritically credits the alarmist accounts of various commissions on the so-called rising tide of mediocrity of American education.[8] He claims that there have been only scattered objections to the various recent commission reports (p. 101). The fact that the reports were bought by the media and the public, I don't think should count as evidence on the validity of their conclusions. In fact, there have been some important criticisms, which I'll touch on below.

Murray also assumes, without, I believe, demonstrating it, that the decline affected the disadvantaged most (p. 108). I think if anything is the case, at least in the kind of quantitative data that are published, the opposite is true.

Murray examines scattered test score data spanning 1960 to 1980, in what is an incomplete and to some extent flawed, analysis (pp. 103-108). There is, again, nothing on specifically inner-city schools, which may be where there is such evidence that might persuade.

The commissions' reports that Murray relies on were polemics that wished to convey a preconceived image of American education. They were, to say the least, rather cavalier with data. Lawrence Stedman and Marshall Smith, two University of Wisconsin-Madison researchers,

concluded after a thorough review of the several reports that "The commissions used weak arguments and poor data to make their case. . . . By ignoring their background reports and carelessly handling data, their reports further lost credibility."[9] Paul Peterson, who is now director of government studies at the Brookings Institution, called the data in these commissions' works "Patchy, dated, not nearly as dramatic as the rhetoric employed,"[10] and concluded "Only through the selective use of uncertain evidence can they make the case that American schools have declined."[11]

For example, on one important piece of data that the commissions treated, and which is relevant to Murray's claims, i.e., a diluted curriculum, it may be worth noting that direct recourse to the 1980 High School and Beyond data and the 1972 National Longitudinal Study of the Class of 1972 data shows that more blacks were in "academic" or college preparatory programs in 1980 than in 1972.[12] That's a little thing, but it's the kind of thing you want to get into in more detail if you're going to believe or if you're going to use these reports and data.

Murray (pp. 103-106) juxtaposes test-score data from 1960 Project TALENT ninth graders (TALENT was a large national sample)[13]; the 1965 Equality of Educational Opportunity Report (EEOR) data (i.e., the earlier Coleman report)[14] for <u>twelfth</u> graders, and 1980 data for a nationally representative sample of youths aged 18 to 23 who were given the Armed Forces Qualification Test (AFQT).[15] Now note: because of selectivity in the process of early school leaving, comparison of twelfth graders with ninth graders is likely to show improvement in achievement or aptitude levels. Because blacks are more likely to leave school before their

senior year, the fact that scores for black twelfth graders in 1965 are closer to white scores then are scores for black ninth graders in 1960 is a predictable artifact of differential drop-out rates. It may or may not reflect underlying improvement.

The chief flaw in Murray's treatment of test scores is his interpreting scores on standardized tests as if they exist on ratio scales. For example, Murray speaks about whites knowing a third more or performing a third better than blacks, in the case of TALENT. He speaks about the 1980 AFQT data as showing that whites score twice as high as blacks (p. 105). But these are not ratio scales. As we all know from taking tests throughout our lives, scores are reported in different metrics and different numbers. There is even some question as to whether they're really interval scales, let alone ratio scales. Murray's approach to this simply does not make sense to me, however much it may conform to lay intuition.

Now, midway through this, it appears that Murray recognizes this may not be the best way to talk about test scores, and he says, in effect, "in some cases, talking about it like this may be meaningless, so I'll talk about standard deviation differences" (p. 104). And he does that, quite sensibly, for TALENT and the Coleman data, but does not do it for the 1980 AFQT data, or the 1980 or 1981 Scholastic Aptitude Test (SAT) data, which he also reports. As Ernest House has noted previously in a review in Evaluation News,[16] this is a rather serious oversight.

Now, it turns out that the standard deviations are in the Appendix tables, so you can extract them and you can see how they compare for the TALENT data from 1960 and the Coleman data for 1965. The other thing you

can do is to utilize the National Longitudinal Survey Class of 1972 data, and the 1980 and 1982 High School and Beyond data if you want, as Murray does, to advance credible claims about trends in the standardized test scores of high school seniors.

I took the liberty of doing this, and have inserted those results with results taken from Murray and also from published Coleman Report data, which I examined in order to include some lower grades as well as grade 12. In Table 1 white/black gaps in standardized test scores are rendered, as close as I could get them, in terms of total standard deviations.

If somebody wants to point the widening trend in those gaps, I would be happy to see it. As matters stand, I do not. Now maybe a standard deviation isn't a standard deviation isn't a standard deviation. Does a standard deviation mean something different in terms of absolute learning, in terms of relative proficiency, in 1980, in these kinds of tests than it did in 1960? Since these tests, especially the Class of 1972 and the High School and Beyond are concocted to as best as possible maintain comparability, I don't think you could argue this. I'd be open to hearing the argument, but I don't know in advance what it could possibly be.

That there was an enormous achievement gap between the races in 1980, that there was a catastrophic difference between blacks and whites in SATs, for example, are things I think that I would be ready to join Murray in saying. To say, however, that there was a <u>worsening</u> trend since 1965 is simply not something for which I find evidence.

There are also better tests, in certain respects, if you want to test what the schools are doing, than the standardized aptitude and achieve-

Table 1

White-Black Gap in Standardized Test Scores
(in terms of total standard deviation (SD))

Year	Sample	Grade	Test	Gap (SD)	Source
1960	Project TALENT	9	Composite	1.28	Murray, Table 13, p. 253
1965	EEOR (Coleman Report)	6	Verbal	1.00	Smith in Mosteller and Moynihan[a]
		9	Verbal	1.00	
		12	Verbal	1.01	
1972	NLS 1972	12	Composite	1.10	Direct calculation
1980	High School and Beyond	10	Composite	0.96	Direct calculation
		12	Composite	0.82	Direct calculation
1980	SAT	11 & 12	Verbal	1.04	Murray, Table 16, p. 255[a]
			Math	1.05	
1980	U.S. Department of Defense	18-19 yrs.	Armed Forces Qualification Test	1.05	Murray, Table 15, p. 254[b]
		20-21 yrs.	AFQT	1.14	
		22-23 yrs.	AFQT	1.20	
1982	High School and Beyond	12	Composite	0.96	Direct calculation
		12 plus dropouts	Composite	0.93	Direct calculation

[a](White mean - black mean)/1.05 SD_W. NLS 1972 and H.S.&B. indicate $SD_{Total} = 1.05\ SD_W$. EEOR data are from Marshall S. Smith, "Equality of Educational Opportunity: The Basic Findings Reconsidered," in F. Mostellar and D. P. Moynihan, eds., On Equality of Educational Opportunity, (New York: Random House, 1972).

[b]Age-specific SDs averaged, and used consistently.

ment tests. One such set of tests are those used in the National Assessment of Educational Progress (NAEP), which has been given periodically in several skill and content areas, for 9-, 13- and 17-year-olds.[17] I have not directly analyzed these data, nor have I exhaustively read the extensive NAEP reports, but I have consulted the reports and several secondary sources to find pertinent results. I found, for example, that between 1970 and 1980, black 9-year-olds gained 10 percentage points in reading.[18] (This is rendered in percent correct; I have not figured out how to convert that into something more interpretable.)

During the 1970s, there were on the whole, minor, where any, declines in writing, science, math, social studies, and reading, among 9- and 13-year-olds, and there were diminishing racial differences. Between the early and mid-1970s, NAEP shows gains in basic literacy among 17-year-olds. The literacy statistics have been taken to show disaster, and if you read them you'd say, "My god, we're a nation of illiterates," but my point is that in the mid-1970s we were less a nation of illiterates than we were in 1971.

Now, to be fair, it's important to note that what I emphasize is for 9- and 13-year-olds. The younger kids seem to be doing fine and they have been doing fine, and contrary to popular impression, the basics are in good order. The older kids, on the other hand, can't think, write, infer, analyze, induce, read, or do much of anything. . . . I exaggerate a bit, but having now taught university for 10 years, I'm not unsympathetic to such grandiose generalizations.

I did extract out some NAEP results specifically for 17-year-olds. Between 1973 and 1978 whites dropped 3.5 percentage points in math and

blacks dropped 2.6. Between 1978 and 1982 whites dropped 0.1, and blacks gained 1.3. Reading: trivial black gains, I mean less than one percent; trivial white losses, 1971-1980. Important to notice: in the Southeast, where most of the integration took place, in the early and mid-1970s, whites gained, this is for 17-year-olds, 2 percentage points in reading, and blacks gained 2.1. This is important because, in passing, Murray implies that in response to racial integration, there was a de-emphasis of academic rewards and academic accomplishment, in order not to show up and make visible white/black contrasts (pp. 174-175). I think this is an interesting bit of data to come out of that. I would also add, however, in support of what he says, the ethnographic things I've seen on integrated schools, whether it's for northern cities, or southern, and there aren't many, I'm thinking of about 2 or 3 studies,[19] show exactly the phenomenon he's talking about, which may again point up either the need to go into school sites, to see what's going on; or to say regardless of what's going on in the black box, it's not showing up in our conventional measures of educational outcomes.

Now, it's important also to contrast the trends in elementary education and basic skills with high school and high-order skills, because the federal initiatives in education were directed primarily at pre-school and elementary education. Despite secondary schools being eligible for Title 1 of Elementary and Secondary Education Act (ESEA) money, the fact is that most of it went into elementary schools. This fact is of significance because Murray's criticisms of federal educational programs are given warrant by trends in the achievement of <u>older</u> students.

Murray's principal concern is not with data, but with what he claims is the flawed logic of the policy reforms and initiatives undertaken during the mid-1960s. At the conclusion of each of the chapters dealing with specific policy areas (i.e., wages and occupations, education, poverty, crime, family structure), Murray presents a capsule summary of federal measures pertaining to the area in question. The implication the reader is invited to draw is that the federal measures listed are in some way responsible for the ills described in the body of each chapter.

At the conclusion of his chapter on education, Murray reviews federally financed interventions and benefits as well as policy directions in desegregation and students' rights (pp. 109-112). Subsequent chapters make it evident that it is principally the latter which Murray associates with educational deterioration in the decade-and-a-half before 1980. Nevertheless, he concludes that "the federal investment of $60 billion in elementary and secondary education bought nothing discernible" (p. 101).

This is an arguable proposition. Dissenters would cite the gains evident in the NAEP data for younger minority students, long-term reductions in grade retardation and special education placements, as well as higher educational attainment among alumni of pre-school programs for the poor, and evidence from the Sustaining Effects study[20] indicating modest short-term gains among the less educationally disadvantaged of the students in Title I programs. My own judgment is that the gains reported are too small, too narrowly distributed, too ephemeral, and too uncertain to confidently reject Murray's conclusion.

Nevertheless, I believe that more plausible accounts may be offered for the failure of compensatory education to have made an appreciable difference to the education of the disadvantaged. One, advanced by Christopher Jencks, is that redundancy in the elementary school curriculum allows students who have not experienced any compensatory programs to catch up to those who have.[21] Other possibilities include trade-offs that "pull out" strategies might impose, such that students pulled out of their classes for supplementary programs miss some regular instruction and potential neglect of low achievers by regular teachers, who presume that Title I will take care of those who progress slowly and have low achievement levels. Finally, compensatory programs may fail to alter the social relationships of the classroom in relevant ways.

Whatever the actual reasons for the minimal benefits conferred by compensatory education programs, I am fairly confident that one factor cited by Murray--i.e., the enlargement of due process rights for students--is a minor part of the story. Based on Henry Lufler, Ellen Jane Hollingsworth and William Clune's empirical research on school discipline and the implementation of Goss (which is the school-related progeny of Gault, the precedent-setting case in juvenile legal rights),[22] I do not believe that teachers' hands are significantly more tied than they were before Goss. The courts have not demanded wholesale reform in the conduct of expulsions and suspensions, and schools appear to be operating pretty much as they had before Goss. Certainly, there has been no ground swell of court actions in this area. It is, of course, possible that teachers feared a greater threat than in fact existed and may, on that account, have been less demanding and less strict with their

charges than in the period before Goss. Having minimized the role of the courts in the schools, I nevertheless want to say, as a sociologist, there was in fact an evident crisis in legitimacy in education during the late 1960s and 1970s; it started in the education profession, especially at the top, with professors and graduate students in elite education schools. I believe, having been a teacher in the late 1960s, that the profession lost confidence in the validity and value of what it was doing, under assaults which accused it of "cultural genocide" and "cultural imperialism," and we lost our willingness to act in the wider sense of the term as authorities, whether pedagogic or disciplinary.

Interestingly, this analysis is shared by the left. I refer you to Christopher Lasch's book The Culture of Narcissism containing his discussion of attacks on academic standards in the name of attacks on eliteism.[23] And it was no conservative who wrote the following words, in response to Nathan Glazer's comments at a Poverty Institute Conference:

> When we look at secondary schools, the gap between what these schools do and what we know how to do seems to be greater. These schools make relatively little effort to teach basic skills to those students who still lack them. Indeed, they make little effort to teach students much of anything; there are no meaningful penalties for not doing academic assignments or for breaking most social rules. As a result, many students do no work and ignore the rules. None of this is inevitable; one can suspend and expel students who do not do what is expected of them. One can also teach basic skills to those students who lack them. Both the decline in test performance in American schools, and the increasing dropout rates are, I would argue, by-products of deliberate decisions not to do these things. These changes were made with the best of intentions, but they don't work and they can now be reversed.[24]

That was Christopher Jencks speaking, as I say, in reply to Glazer. So I think there's a consensus, at least in certain circles there is no difference here between the left and the right. And, interestingly, it

was also the leftist Center for the Study of Public Policy, in Cambridge, that in the early 1970s proposed educational vouchers for the poor.[25] An idea, I would say, that may well be worthy of experiment and which might even have the effects which Charles Murray predicts.

Notes

[1]Alice Rivlin, "Forensic Social Science," Harvard Educational Review, 43 (February 1973), 61-75.

[2]Daniel P. Moynihan, "The Schism in Black America," Public Interest, 27 (spring 1972), 3-24.

[3]Finis Welch, "Black-White Differences in Return to Schooling," American Economic Review, 63 (December 1973), 893-907.

[4]Calculated from W. Vance Grant and Thomas D. Snyder, Digest of Education Statistics 1983-1984 (Washington, D.C.: National Center for Education Statistics, n.d.), Tables 8 (p. 3) and 59 (p. 71).

[5]Ibid., Table 8, p. 13.

[6]See, for example, W. Lee Hansen, "Economic Growth and Equal Opportunity: Conflicting or Complementary Goals in Higher Education?" In Edwin Dean, ed., Education and Economic Productivity (Cambridge, Mass.: Ballinger, 1984).

[7]Reynolds Farley, Blacks and Whites: Narrowing the Gap? (Cambridge, Mass.: Harvard University Press, 1984), Table A3.

[8]The "rising tide of mediocrity" quotation is taken from National Commission on Excellence in Education, A Nation at Risk: The Imperative for Educational Reform (Washington, D.C.: GPO, 1983). For similar critiques see Task Force on Education for Economic Growth, Action for Excellence: A Comprehensive Plan to Improve Our Nation's Schools (Denver: Education Commission of the States, 1983); and Twentieth Century Fund Task Force on Federal Elementary and Secondary Education Policy, Making the Grade (New York: Twentieth Century Fund, 1983).

[9]Lawrence C. Stedman and Marshall S. Smith, "Recent Reform Proposals for American Education," Contemporary Education Review, 2 (fall 1983), 102.

[10]Paul E. Peterson, "Did the Education Commissions Say Anything?" Brookings Review (winter 1983), p. 4.

[11]Ibid., p. 5.

[12]Stedman and Smith, p. 90.

[13]Project TALENT was a large national longitudinal survey of students in a representative stratified sample of almost 1000 schools, first conducted by the American Institutes for Research in 1960. Murray draws data from Lauress L. Wise, Donald H. McLaughlin, and Kevin J. Gilmartin, The American Citizen: Eleven Years after High School (Palo Alto, Calif.: American Institutes for Research, 1977).

[14]James S. Coleman, Ernest Q. Campbell, Carol J. Hobson, James McPartland, Alexander M. Mood, Frederic D. Weinfeld, and Robert L. York, Equality of Educational Opportunity (Washington, D.C.: GPO, 1966).

[15]Profile of American Youth (Washington, D.C.: Office of the Assistant Secretary of Defense, Manpower Reserve Affairs and Logistics, 1982).

[16]Ernest R. House, "Undisciplined Social Science," Evaluation News, 6 (May 1985), 28-31.

[17]The National Assessment, conducted until recently by the Educational Commission of the States (now by the Educational Testing Service), periodically tests representative samples of 9-, 13-, and 17-year-olds in a variety of subject and skill areas, and issues numerous reports detailing the results. See, for example, Three National Assessments of Reading: Changes in Performance, 1970-1980 (Denver: Educational Commission of the States, Report No. 11-R-01, April 1981).

[18]Benjamin D. Stickney and Virginia R. L. Plunkett, "Closing the Gap: A Historical Perspective on the Effectiveness of Compensatory Education," Phi Delta Kappan, 65 (December 1983), p. 290.

[19]See, for example, Dorothy C. Clement, Margaret Eisenhart, and Joe R. Harding, "The Veneer of Harmony: Social-Race Relations in a Southern Desegregated School," in Ray C. Rist, ed., Desegregated Schools: Appraisals of an American Experiment (New York: Academic Press, 1979), and Mary Haywood Metz, "Sources of Constructive Social Relationships in an Urban Magnet School," American Journal of Education, 91 (February 1983), 202-245.

[20]See Launor F. Carter, "The Sustaining Effects Study of Compensatory Education," Educational Researcher, 13 (August/September 1984), 4-13.

[21]Christopher Jencks, "Nathan Glazer's 'Education and Training Programs and Poverty,'" unpublished paper prepared for Institute for Research on Poverty conference, Poverty and Policy: Retrospect and Prospects, Williamsburg, Va., December 6-8, 1984, pp. 7-8.

[22]Ellen Jane Hollingsworth, Henry S. Lufler, and William H. Clune, School Discipline: Order and Autonomy (New York: Praeger Publishers, 1984).

[23]Christopher Lasch, The Culture of Narcissism: American Life in an Age of Diminishing Expectations (New York: Norton, 1978).

[24]Jencks, pp. 13-14.

[25]See Education Vouchers, A Report on Financing Elementary Education by Grants to Parents (Cambridge, Mass.: Center for the Study of Public Policy, 1970).

Irving Piliavin: The 1965-1970 Crime Increase as Seen by Charles Murray: A Critique

Among the policy concerns addressed by Charles Murray in Losing Ground is the persistent, and large, increase in U.S. crime rates during the period from 1960 to 1980. Murray claims that this increase, particularly that portion occurring between 1965 and 1970, can be explained by a series of events that took place within the criminal justice system during roughly the same period. I believe Murray's data, as well as other statistics, fail to support his thesis.

I shall first summarize the thesis as I understand it and then comment on it.

Murray claims that for some 10 to 15 years prior to 1964 or thereabouts--the year varies by offense--crime rates showed only minor annual increases and, in the case of homicides between 1960 and 1965, essentially held steady. Then, in the mid-1960s, there was a sudden upsurge in crime rates. In the key years 1965 to 1970 violent crime increased 81 percent and property crime increased 61 percent (p. 256). After 1970, crime rates continued to increase substantially, but not as steeply as during the 1965-1970 period. How did this sudden upsurge in crime come about, and why is the interval between 1965 and 1970 so important?

The crucial factors in the crime rate increase, says Murray, are changes in the operation of the criminal justice system that lowered the risks of apprehension and incarceration for those who committed crime.

The significance of the period 1965-1970 is found in the fact that this was the time when these changes were coming into full bloom. The specific modifications that Murray identifies are the following:

1. The odds of going unarrested for five crimes improved. This is measured as $\left(\frac{1}{1 - CR_i}\right)^5$, where CR_i is the clearance rate for year "i".[1] Murray reports that for robbery, a person having committed five crimes had only one chance in fifteen of escaping arrest in 1950. He had one chance in five in 1978. The odds of remaining arrest free in the case of burglary went down from about 6 to 1 to less than 3 to 1 in the same period (p. 168).

2. The odds of imprisonment if arrested by police declined. These odds are measured as the ratio, each year, of prisoners in state and federal correctional facilities to the number of index crime arrests ("index" refers to the types of crimes that compose the two categories, violent crime or property crime, used in crime statistics). According to Murray, this ratio went from about one in four in 1960 to one in eight in 1980. In 1975, it was less than one in ten (p. 169).

3. Protective regulations were extended for those arrested. In addition to these alterations in the risks associated with crime, Murray identifies changes in procedural rules concerning offenders that he claims further served to increase the likelihood of crime. For adult offenders, these changes involved restrictions on the conditions under which police could obtain confessions and retain suspects in custody. In the case of juveniles, the restrictions involved as well the tightening of access to their records as juveniles when they became adults. By 1974 one-third of the states had legislation requiring that such records be expunged.

All of these changes, says Murray, lessened the risks and severity of punishment for crime facing potential offenders, particularly those who are poor. Some of the changes--those granting adults equal protection under the law--are recognized as good by Murray, and presumably he believes they should remain. The other changes, again presumably, he believes are in need of revision.

It is important to note that Murray's specific concern is with crime among the poor. It is his contention that the poor benefited more from the aforementioned criminal justice alterations than did other income groups in the United States and that as a consequence, crime rates among the poor increased more than among the nonpoor, particularly between 1965 and 1970.

The data which Murray uses to describe crime and its potential costs among U.S. citizens do not refer directly to those who are poor and those who are not poor; they instead concern those who are nonwhite and white. These two groups constitute, for Murray, proxies for the two income classes. Using national arrest-rate data as indicators of crime rates, Murray shows that the increase over the period 1965-1970 in violent crimes (arrests) among the poor (nonwhites) was seven times that among the nonpoor (whites). Furthermore, he notes, 91 percent of the increase in violent crimes of the poor (nonwhite) between 1960 and 1980 took place in the years 1965 to 1970, while only 29 percent of the increase in violent crimes among the nonpoor (whites) occurred during those years. Thus, says Murray, the crime rate of the poor "... got worse very suddenly, over a very concentrated period" (p. 118). This is the same period in which, not coincidentally, the crime-enhancing modifications in the criminal justice system took place.

My points are as follows: (1) the crime-rate increase among the "poor," as measured by nonwhite arrests, was in a very important sense not larger in magnitude than that of the "nonpoor," as measured by white arrests; (2) that the crime rate among white poor did not increase suddenly and exclusively over the period in which Murray locates the crime-rate increase among the "poor" (nonwhites); and (3) the relationships reported by Murray between increases in crime rates among the poor and events in the criminal justice system are more apparent than real.

THE CRIME RATE INCREASE AMONG THE POOR AND NONPOOR

Table 1 displays data on the arrests and arrest rates of whites and nonwhites (Murray's proxies respectively for the nonpoor and poor) at five-year intervals, 1960 to 1980. The rates shown differ from those presented by Murray because I use, as population bases, white and black men aged 15 to 24 (who constitute those most likely to commit crimes), while Murray employs white or black individuals of both sexes and all ages. Murray and I both find, however, quite similar time trends.

Paralleling Murray's results is an absolute increase in the nonwhite arrest rate for violent crime between 1965 and 1970 of over six times that of whites. However, the relative increase of the nonwhite arrest rates was less than that of whites, 34.7 percent vs. 47.5 percent. Furthermore, with but one exception, the relative increase in white arrest rates for violent and property crime was comparable to or greater than that among nonwhites at each of the five-year intervals between 1960 and 1980. Overall, between 1960 and 1980 the relative increase in white arrest rates was 30 percent higher than that of nonwhites for property

Table 1

Population Size, Arrests, and Arrest Rates for Violent and Property Index Crimes per 100,000 Civilian Male Population, Aged, 15-24, By Race, 1960-1980

Year	Whites				Blacks and Others			
	Population (in millions)	Arrests		% Change in Rate from Preceding Period	Population (in millions)	Arrests		% Change in Rate from Preceding Period
		Number	Rate (per 100,000)			Number	Rate (per 100,000)	
			Violent Crimes					
1960	9.66	39,369	407	—	1.37	51,081	3,728	—
1965	12.32	56,285	457	+ 12.3%	1.73	69,691	4,028	+ 8.1%
1970	14.25	96,100	674	+ 47.5%	2.19	118,803	5,425	+ 34.7%
1975	16.77	154,384	921	+ 36.7%	2.75	134,444	4,889	- 9.8%
1980	17.52	210,207	1,200	+ 30.3%	3.13	179,058	5,720	+ 17.0%
1965-1980				+ 195.0%				+ 53.4%
			Property Crimes					
1960	9.66	280,041	2,899	—	1.37	126,861	9,260	—
1965	12.32	429,973	3,490	+ 20.4%	1.73	208,636	12,059	+ 30.2%
1970	14.25	640,968	4,498	+ 28.9%	2.19	340,272	15,537	+ 28.8%
1975	16.77	936,551	5,585	+ 24.2%	2.75	397,577	14,457	- 6.85
1980	17.52	1,017,703	5,809	+ 4.0%	3.13	482,607	15,419	+ 6.7%
1965-1980				+ 100.4%				+ 66.5%

Source: Federal Bureau of Investigation, Crime in the United States (Washington, D.C.: GPO, 1960-1980).

Note: "Index crime" refers to the types of crime that make up the two categories. Violent crime includes murder, rape, robbery, and aggravated assault. Property crime includes burglary, larceny, and auto theft.

crimes and more than 300 percent higher than that of nonwhites for violent crimes.

Why do I emphasize the relative rather than absolute increases in white and nonwhite crime rate increases? First, as Murray himself notes, nonwhites have historically had higher crime rates than whites. It is very likely that whatever factors contributed to the generation of these rates in the past continue to be relevant to present nonwhite crime rates. Thus marginal increases in these rates can be expected to remain larger than marginal increases in white rates in the same proportion as the relative size of white and nonwhite crime rates. One of the clearly relevant factors in determining these rates is poverty itself. Murray acknowledges the importance of this factor in his use of nonwhites to proxy the poor. Yet, it is inappropriate to base crime-rate estimates on the assumption, as does Murray, that white arrests reflect crimes committed by the nonpoor, while nonwhite arrests index crimes of the poor. In fact, it is likely that white arrest rates proxy crime rates of poor whites and that nonwhite arrest rates capture the crime rates of poor nonwhites. Let us assume this is true and in fact posit that crime (of the types considered here) within each racial group is located solely among the poor.[2] Our interest is in the crime rates of poor whites and poor nonwhites. Table 2 presents estimates of these crime rates, based on the arrest data reported in Table 1, but using as its population basis the number of poor white and nonwhite crime-aged men as estimated from Census Bureau data.[3]

The data indicate that the poor were not of a piece in their crime rate patterns during the two decades between 1960 and 1980. While poor nonwhites, as pointed out by Murray, largely confined their increases in

Table 2

Population Size, Arrests, and Arrest Rates for Violent and Property Index Crimes per 100,000 Civilian Male Population in Poverty, Aged 15-24, By Race, 1960-1980

Year	Whites				Blacks and Others			
	Population (in millions)	Arrests Number	Arrests Rate (per 100,000)	% Change in Rate from Preceding Period	Population (in millions)	Arrests Number	Arrests Rate (per 100,000)	% Change in Rate from Preceding Period
			Violent Crimes					
1960	1.37	39,369	2,874	—	.53	51,081	9,638	—
1965	1.22	56,285	4,613	+ 60.5%	.66	69,691	10,560	+ 9.6%
1970	1.13	96,100	8,504	+ 84.3%	.65	118,803	18,277	+ 73.1%
1975	1.53	154,384	10,090	+ 18.6%	.79	134,444	17,018	- 6.9%
1980	1.58	210,207	13,304	+ 31.9%	.82	139,058	16,958	+ 0.0%
			Property Crimes					
1960	1.37	280,041	20,400	—	.53	126,861	23,936	—
1965	1.22	429,973	35,244	+ 72.8%	.66	208,636	31,611	+ 32.1%
1970	1.13	640,968	56,722	+ 60.9%	.65	340,272	52,349	+ 65.6%
1975	1.53	936,551	61,212	+ 7.9%	.79	397,577	50,326	- 3.9%
1980	1.58	1,017,703	64,441	+ 5.3%	.82	482,607	58,854	+ 16.9%

Sources: Table 1, adjusted by Census Bureau data on the poverty population (described in note 3).

violent crime to the 1965-1970 period, poor whites significantly increased at every observation point the rate at which they engaged in violent crime. Although poor whites were committing violent crime at 44 percent of the rate of poor nonwhites in 1965, they were at 78 percent of the poor nonwhite rate in 1980. Poor nonwhites and poor whites experienced massive increases in property crime rates between 1965 and 1970, but both groups also had substantial increases (more so among whites) between 1960 and 1965, a period preceding the criminal justice changes that Murray believes led to the crime explosion of the late 1960s.

In presenting the estimates in Table 2 I do not mean to imply that white and nonwhite crimes are committed solely by the poor. The data there are presented in order to convey two points: To the extent that crime among whites and nonwhites is committed by the poor, the crime rate increases noted by Murray (p. 118) are grossly underestimated, particularly among whites;[4] and, despite Murray's suggestion that the period 1965-1970 ushered in criminal justice changes that lessened the risks of crime for the poor, poor whites increased their rates of violent and nonviolent crime before as well as after the changes were carried out.

POLICY CHANGES AND CRIMINAL ACTIVITY

It is not clear whether Murray believes that the decreases in risks of criminal activity were the result of policy changes in the criminal justice system. At one point in his volume he professes not to assign blame ("The question of causation does not arise," p. 169). Yet the tenor of Losing Ground is to suggest that the crime problem of the 1970s,

among others, reflects the failure of changed social policies ("A government's social policy helps set the rules of the game," p. 9). Whatever Murray's true opinion may be, his estimation of the size and character of the decreased risks associated with crime is both exaggerated and misleading. Furthermore, if his remarks might be taken to imply that criminal justice policies were responsible for these decreased risks and that these decreased risks actually contributed to the growth of crime rates, such implications are not justified.

First, let us examine the character of the risks associated with crime that Murray calls to our attention. He compares the situations in 1960 and 1980 in the following terms: "A youth hanging out on a tough urban street corner in 1960 was unlikely to know many (if any) people who could credibly claim to have gotten away with a string of robberies; in 1970, a youth hanging out on the same street corner might easily know several" (p. 169). The accuracy of the assertion is open to question. The probability of "getting away" with five robberies in 1965 according to Murray's data (p. 168) was somewhat less than one in eleven; in 1970, it was somewhat less than one in five. If the corner youth in 1960 had six friends, all of whom committed five robberies, the probability of at least one still being on the corner was .43. In 1970 the probability increased to .69, an increase indeed, but not of the magnitude implied by Murray. More important, the sampling frame used by Murray is misleading. It might be concluded from his argument that after 1960 more people, presumably young and poor, were escaping arrest for crime. This is not true. In fact at each five-year observation from 1960 through 1975, police arrested more individuals and at a higher rate per 100,000

population (termed "unit population") than at previous observation points. In 1960, the police arrest rate per unit population was 0.3 percent; it subsequently was 0.4 percent (1965); 0.5 percent (1970); 1.1 percent (1975); and 1.0 percent (1980).[5] Arrest rates for youth and poor youth populations show similar trends as seen from Tables 1 and 2. These increases in arrests were associated with an increase in police manpower and productivity through the 1960s and 1970s. As seen in Table 3, the concentration of police per 1,000 persons increased 23 percent from 1960 through 1975 and the arrest rate per police officer increased 46 percent. Between 1960 and 1975, and in particular between 1965 and 1970, more people per unit population were being arrested, more police were put on duty, and each police officer was arresting more people. Yet, as Murray and I have found, crime rates throughout this period substantially increased. Seemingly, despite increasing risk of arrest, more individuals were increasing the rates at which they engaged in crime.[6] But why? Murray offers another possibility: the nationwide decline in the imprisonment of arrested offenders. But is it the case that decreases in the likelihood of going to prison led to increases in crime rates? I focus my analysis on the years 1965 to 1970, the key period according to Murray. Rather than rely on national statistics, I employ state data on crime rates for 1965 and 1970 and state prison sentencing practices from 1965 through 1970. The data are shown in Table 4. For economy, I use data only from selected states, chosen on the basis of their size and geographic location.

First note that one state, California, had a relatively low increase in crime rates between 1965 and 1970 even though it had, in 1965, a low

Table 3

Police Density and Arrest Activity, 1960-1980

Year	Number of Police Officers	Number of Police per 1,000 Inhabitants	Number of Arrests per Police Officer
1960	175,598	1.7	22.5
1965	190,005	1.7	26.1
1970	233,562	2.0	27.8
1975	292,346	2.1	27.3
1980	294,181	2.1	32.9

Source: FBI, Crime in the United States (Washington, D.C.: GPO, 1960-1980).

Table 4

Index Crime Rates and Prison Sentences by Courts,
Selected States, 1965-1970

State	Index Crime Rates[a] 1965	1970	% Change 1965-1970	Prison Sentencing: Sentences in 1965 Number	as % of Index Crimes	Sentences 1966-1970: 1966	1967	1968	1969	1970	% Change 1965-1970 in Sentenced Offenders
Ala.	1,068	1,865	+ 74.6%	1,873	5.1%	1,565	1,481	1,639	1,740	1,469	- 21.6%
Cal.	2,644	4,307	+ 63.0%	6,004	1.2%	5,525	5,144	4,949	4,754	4,690	- 21.9%
D.C.	2,171	4,111	+ 89.4%	667	1.3%	626	501	1,265	—	1,196	+ 79.3%
Fla.	2,010	3,600	+ 79.1%	3,193	2.9%	2,831	2,988	3,610	3,778	4,395	+ 37.6%
Ill.	1,613	2,347	+ 45.5%	2,461	1.4%	2,067	2,232	2,332	2,630	2,428	- 1.3%
La.	1,184	2,404	+ 103.0%	1,960	4.7%	1,743	1,449	1,714	1,689	1,627	- 17.0%
Mich.	1,734	3,659	+ 111.0%	3,280	2.3%	3,088	3,719	2,400	2,614	3,180	- 3.0%
N.Y.	1,608	3,922	+ 143.9%	5,474	1.9%	4,732	4,686	4,223	4,358	4,134	- 24.5%
Ohio	1,039	2,078	+ 100.0%	3,421	3.2%	3,251	3,417	3,757	3,932	4,083	+ 19.4%
Penn.	968	1,541	+ 59.2%	2,015	1.8%	1,873	1,598	—	1,822	1,955	+ 28.5%
Tex.	1,403	2,706	+ 92.9%	5,615	3.8%	4,921	5,020	4,833	5,359	6,610	+ 17.7%
Utah	1,394	2,373	+ 70.2%	302	2.5%	194	206	198	206	204	- 32.5%
Wis.	738	1,514	+ 105.1%	1,199	3.9%	1,149	1,199	1,379	1,191	1,371	+ 14.3%

Source: FBI, Crime in the United States (Washington, D.C.: GPO, 1965-1970); Law Enforcement Assistance Administration, National Prisoner Statistics: Prisoners in State and Federal Institutions (Washington, D.C.: GPO, 1965-1970).

[a]"For both violent and property crimes; see note to Table 1."

ratio of prison sentences relative to crimes committed and then steadily reduced the number of sentences to prison through 1970. Note also that Texas and Ohio, initially having comparatively high rates of imprisonment relative to crimes committed, still encountered higher crime rate increases than California, despite generally upward trends in their number of prison commitments. Other state trends in Table 4 also seem to contradict the Murray thesis. Illinois, for example, which had the lowest relative increase in crime rates between 1965 and 1970, had an initially low ratio of sentences to crimes committed, and made no major changes in prison sentencing over the five years examined. The major point of all this is that if the 1965-1970 crime rate increases were due to changes in prison-sentencing practices during this period, the relationship is far from obvious.

What then can we conclude? I have not intended to refute the hypothesis that variation in the risks and severity of criminal sanctions are irrelevant to crime rates. The conditions under which these variations may be relevant is a topic beyond the scope of this presentation. What I have hoped to show is that Murray is simply wrong in contending that there was a uniquely severe increase in crime among the poor between 1965 and 1970, or that whatever crime rate increases took place after 1965 can be easily attributed to changes in criminal justice practices.

The basic problem with Murray's analysis is that it is casual. He has not discovered the phenomena responsible for the crime rate increases of the 1960s and 1970s. On the other hand, I do not know anyone who has.

Notes

[1]The clearance rate refers to the percentage of crimes of any given type--robbery, burglary, etc.--which are solved by arrest.

[2]This is a harmless premise unless we assume that (1) crime does not have the same class distribution among whites and nonwhites, or (2) that nonclass factors operate differently among whites and nonwhites. Since Murray does not make these assumptions, neither shall I. Of course, to the extent the last assumption is correct, Murray's argument requires substantial revision.

[3]These percentages of 15- to 24-year-old men in poverty are based on information provided in Current Population Reports, Series P-60, "Consumer Income," of the U.S. Census Bureau. The 1975 and 1980 data were taken directly from Table 11 of P-60, no. 106 and no. 133. The 1960, 1965, and 1970 data were based on extrapolations from Table 1 of P-60, no. 54 (1960, 1965) and Table 5 of P-60, no. 81 (1970). The 1960 and 1965 extrapolations are the most tenuous because the relevant data for these years refer only to children under 18 related to household heads by blood, marriage, or adoption. In converting the information to the table entries, I assume that the poverty rates of males 15 to 24 years of age were 73 percent of the rates given for related children under 18 years of age. The actual rates found for years 1970, 1975, and 1980 were 72 percent, 67 percent, and 75 percent, respectively.

[4]To the extent that violent and property crime among whites and nonwhites is committed by the poor, and to the extent it is committed equally among whites and nonwhites, the crime rates among poor whites are

very high, have been catching up with the rates among nonwhites, and most important, do not reflect the pattern of crime rates among poor nonwhites.

[5]Population data were obtained from U.S. Census Reports for the given years, police arrest data are those shown in Tables 1 and 2 of this review.

[6]The validity of the above argument would be enhanced if it were backed by local arrest data and police manpower statistics. These are not available to me. As it stands the argument is weakened by its reliance on "ecological correlations," a problem which, incidentally, abounds in Murray's data. By the term "ecological correlations" I refer to correlations observed among aggregated data. These do not necessarily represent correlations at the individual level.

Sheldon Danziger and Peter Gottschalk:
Social Programs - A Partial Solution to, but Not a Cause of Poverty:
An Alternative to Charles Murray's View*

To read the popular press, one would think that Charles Murray had hit a home run. His Losing Ground: American Social Policy, 1950-1980 has drawn rave reviews in a variety of newspapers and journals and its findings are cited in presidential speeches. Murray is to the second Reagan administration's social policy what George Gilder was to the first. In fact, Meg Greenfield noted that his name has assumed special meaning in the policy lexicon:

> No matter what kind of government effort you may argue for these days in this area (social programs), and no matter what obligation, be it ever so modest, you may say the government should assume, you are likely to be "Charles Murrayed," and that will be the end of the argument.[1]

What accounts for this impact? Has he completed the definitive evaluation of War on Poverty and Great Society programs and policies? Are his conclusions so clear that we can dismantle social programs without hurting the poor? Murray presents no new data, but he does review many previous studies and concludes that antipoverty programs have not worked and cannot work:

> We tried to provide more for the poor and produced more poor instead. We tried to remove the barriers to escape from poverty, and inadvertently built a trap (p. 9).

This view is only the latest put forward by a long line of critics of social welfare programs. For example, De Tocqueville's 1835 "Memoir on

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*This paper is in large part from the authors' article, "The Poverty of Losing Ground," in Challenge, May-June 1985, pp. 32-38.

Pauperism," which is based on his analysis of the English welfare system, argued that

> . . . any permanent, regular, administrative system whose aim will be to provide for the needs of the poor, will breed more miseries than it can cure, will deprave the population that it wants to help and comfort . . . .[2]

Murray has been dubbed "the thinking man's George Gilder" because he provides tables and references where Gilder relies primarily on assertions. However, Murray's antipoverty policy prescription--the elimination of all social programs except unemployment insurance for the working-aged population--cannot be logically derived from the facts he presents. While some of the facts are clear enough--social spending today is much higher in real terms and as a percentage of the Gross National Product than it was twenty years ago, and poverty as officially measured is about the same today as it was twenty years ago--they do not support Murray's conclusions.

We review Murray's facts, supplement them with additional evidence, and come to very different interpretations and policy conclusions. We confine our attention to Murray's discussion of the effects of social policies on poverty, work effort, and family stability; our emphasis is on the effects of income transfer programs, because this discussion forms the basis for his central social policy message. Murray discusses a much broader range of topics. These include the role of the attitudes held by the poor themselves, the "elite wisdom" concerning individual versus societal causes of poverty, and the effects of these attitudes and of social policies on crime rates, illegitimate birth rates, and educational attainment. Some of these views are not susceptible to empirical tests; others are, but are not central to the issues discussed here.

To show how our analysis differs from Murray's, we first present a broad overview of his and our interpretation of the facts. This is followed by a review of the trend in poverty and income transfers and the microeconomic evidence on incentives. We conclude with an agenda for the reform of antipoverty policy.

THE BROAD PICTURE

Murray states that during the 1950s and early 1960s poverty, as officially measured, declined rapidly, while transfers grew only slowly. This decline halted just as the rate of growth of transfers quickened during the late 1960s and 1970s. He then suggests that latent poverty (Murray's name for the Poverty Institute's measure of poverty before transfers) is the best indicator of the underlying factors affecting poverty and notes that it has been rising since the late 1960s.

Murray concludes that the observed rise in pretransfer poverty resulted from the disincentives associated with increased transfers. He implicitly assumes that a $1 increase in transfers reduces pretransfer incomes by more than $1. Transfers are thus self-defeating; they create the perfect poverty trap.

The policy implication is clear--"cut the knot, for there is no way to untie it" (p. 228). Because trying to provide for the poor produces more poverty, eliminating transfers will ultimately reduce poverty. One can be pro-poor and still advocate retrenchment, and even elimination of major programs.

Our interpretation of the facts differs substantially. Posttransfer poverty decreased during the 1950s and 1960s because the economy was

strong and transfers were increasing. When the economy weakened, poverty reductions continued for those groups, such as the elderly, whose transfers continued to grow. For others, not as well-protected by income transfers, poverty grew. The increased transfers did produce some disincentives. But the most careful studies find their effects on poverty to be much less important in recent years than the poverty-increasing effects of exogenous economic and demographic factors. Hence, we view the growing gap between pretransfer poverty and poverty after transfers as evidence of the increased importance of the increased transfers in reducing poverty. Eliminating programs would increase poverty.

TRENDS IN POVERTY AND INCOME TRANSFERS

The pretransfer poverty rate is an indicator of the extent to which market forces leave some households in poverty. Column 1 of Table 1 shows that the proportion of persons who were pretransfer poor declined from 21.3 percent in 1965 to a low of 17.7 percent in 1969 and then increased to a high of 24.2 percent in 1983. Our explanation for this decline in the ability, or willingness, to obtain market income differs markedly from Murray's.

Murray's explanation--that increased transfers induced declines in work effort, thereby increasing pretransfer poverty--does not fit the facts. The turning points in pretransfer poverty (Column 1) and transfers (Columns 3 and 4) just do not line up. There is no apparent shift in transfers in 1969, the year that pretransfer poverty started to increase. Indeed, cash plus in-kind transfers per household grew by about 35

Table 1

Poverty, Transfers, and Unemployment

Year	Pretransfer Poverty Incidence (1)	Official (Posttransfer) Poverty Incidence (2)	Real Cash Transfers per Household[a] (1972 dollars) (3)	Real In-Kind Transfers per Household[a] (1972 dollars) (4)	Unemployment Rate (5)
1950	n.a.	n.a.	$ 365	$ 29	5.3%
1955	n.a.	n.a.	460	31	4.4
1960	n.a.	20.2%	664	40	5.5
1961	n.a.	21.9	730	43	6.7
1962	n.a.	21.0	770	49	5.5
1963	n.a.	19.5	791	54	5.7
1964	n.a.	19.0	801	58	5.2
1965	21.3%	17.3	816	63	4.5
1966	n.a.	15.7	878	71	3.8
1967	19.4	14.3	891	150	3.8
1968	18.2	12.8	911	204	3.6
1969	17.7	12.1	958	231	3.5
1970	18.8	12.6	1,010	242	4.9
1971	19.6	12.5	1,150	273	5.9
1972	19.2	11.9	1,225	304	5.6
1973	19.0	11.1	1,272	320	4.9
1974	20.3	11.2	1,263	327	5.6
1975	22.0	12.3	1,395	386	8.5
1976	21.0	11.8	1,513	427	7.7
1977	21.0	11.6	1,508	452	7.1
1978	20.2	11.4	1,488	464	6.1
1979	20.5	11.7	1,419	472	5.8
1980	21.9	13.0	1,414	482	7.1
1981	23.1	14.0	1,458	505	7.6
1982	24.0	15.0	1,475	508	9.7
1983	24.2	15.2	1,543	n.a.	9.6

Sources: For pretransfer poverty, computations by authors from March Current Population Survey data tapes; for consumer price index, and unemployment rate, 1984 Economic Report of the President; for cash and in-kind transfers, "Social Welfare Expenditures under Public Programs in the U.S.," Social Security Bulletin, December 1968, December 1972, January 1971, January 1977, November 1981; for official poverty incidence and number of households, Current Population Reports, Series P-60, "Consumer Income."

[a]Transfers are divided by all households, not by recipient households.

n.a. = not available.

percent in real terms between 1965 and 1969, when pretransfer poverty was declining, and by about the same percent between 1969 and 1974, when it was increasing. Murray's view is also called into question by the recent experience. Real transfers were about the same in 1977 and 1983, yet pretransfer poverty increased rapidly.

The data are more consistent with the view that emphasizes changes in labor demand. As unemployment (Column 5) dropped between 1965 and 1969, pretransfer poverty declined. Since then, unemployment and pretransfer poverty have trended upward. Throughout the 1970s, the poverty-increasing impact of rising unemployment was offset by rising transfers. When transfers stopped growing and unemployment continued to rise, the official poverty rate rose by 1983 to the level of the late 1960s.

The progress against poverty that had been achieved during the War on Poverty - Great Society period was the victim of weakened economic conditions and transfer retrenchment in the 1980s. Nonetheless, the antipoverty effect of transfers is greater today than in the mid-1960s--cash transfers removed 5.1 percent of all persons from poverty in 1967, but 9.1 percent in 1981, two years in which the official rate was about 14 percent (subtract Column 2 from Column 1). A poverty series that values in-kind transfers (data not shown) reveals an even larger increase in the antipoverty effectiveness of all transfers over the past 15 years.

The data in Table 2, which show the trend in poverty for all persons and for six demographic groups, is evidence that "throwing money at problems" can have a dramatic impact. The expansion and indexation of Social Security and the introduction of the Supplemental Security Income program, which accounted for most of the increased transfers over the

Table 2

Official Incidence of Posttransfer Poverty, for Persons Classified by Demographic Group of Household Head

	All Persons (1)	Aged		Nonaged			
		Whites (2)	Nonwhites (3)	White Men with Children (4)	Nonwhite Men with Children (5)	White Women with Children (6)	Nonwhite Women with Children (7)
1967	14.3%	27.0%	52.0%	7.5%	28.4%	38.2%	68.5%
1980	13.0	13.2	35.7	7.8	16.9	39.1	58.3
% Change							
1967-1980	-9.1	-51.1	-31.3	+4.0	-40.5	+2.4	-14.9

Source: Computations by the authors from March Current Population Survey data tapes.

past twenty years, have led to large reductions in poverty among the aged (Columns 2 and 3).

These disaggregated data also refute Murray's conclusion that spending growth did more harm than good for blacks. While poverty rates in each year for nonaged nonwhites with children are higher than for similar whites, the differentials have narrowed substantially--the rates declined for nonwhites, but increased for whites. In 1967, persons living with nonwhite men were four times as likely to be poor as similar whites (Columns 4 and 5). By 1983, the ratio had fallen to two to one. Much of this decline is due to the more rapid increase in the market incomes of black men than white men, an increase that more than offsets the effect of blacks' relatively larger decline in labor force participation which Murray emphasizes. The ratio of poverty rates of nonwhite women with children to that of white women (Columns 6 and 7) fell from 1.8 to 1.5 over the same period. This is certainly not evidence that poor blacks were disproportionately harmed as a result of War on Poverty and Great Society programs.

The fact that nonwhites have become increasingly more likely to live in households headed by women with children than have whites means that aggregate black-white poverty comparisons obscure the advances shown in Table 2. For example, the ratio of poverty rates for all blacks to all whites has remained relatively constant. Murray relies on the aggregate data (see his Figure 4.4) to support his contention that there has been little progress against poverty for blacks. But his conclusion follows only if a substantial portion of the increased percentage of families headed by women was caused by increased government transfers, a result rejected by the literature reviewed below.

THE SPECIAL CASE OF WELFARE SPENDING AND POVERTY

Losing Ground emphasizes the poverty-increasing effects of welfare programs, especially Aid to Families with Dependent Children (AFDC) and Food Stamps. It was the expansion of these two programs between 1960 and 1970 that keep Phyllis and Harold, Murray's representative individuals, from marrying and which lead Harold to work only periodically. Even if one accepts Murray's description of the choices faced by Phyllis and Harold, it does not discredit the broad array of income maintenance programs, since Phyllis and Harold represent a small minority among the poor.[3] As Table 3 shows, AFDC and Food Stamp spending on those who are neither aged nor disabled made up only 7.3 percent of total social welfare expenditures in 1970, a figure dwarfed by the 17 percent share of Medicare and Medicaid and the 57.8 percent share of nonmedical programs for the elderly. Murray discusses neither the positive effects of transfers on poverty among the elderly nor of medical care spending on health, yet such spending accounts for about three-quarters of the total.

In any case, trends in welfare spending challenge the Phyllis and Harold story. Column 1 of Table 4 shows a large increase in the real values of the combined AFDC and Food Stamp guarantee between 1960 and 1972. This rise provides the basis for the increased negative family and work effects Murray attributes to social programs. However, the real guarantee declined just as rapidly during the 1970s, falling below $7,000 by 1984, and implying reduced disincentives for Harold and Phyllis. Yet

Table 3

Costs of Major Income Transfer Programs for the Elderly, Totally Disabled and All Others

	1960	1970	1980
	(Billions of 1980 $)		
Programs for the Elderly			
Medicare - Medicaid	$ 0.0	$ 21.1	$ 37.8
All Other Programs	45.3	94.6	148.7
Programs for the Totally Disabled			
Medicare - Medicaid	0.0	2.2	11.5
All Other Programs	2.3	9.6	20.9
Programs for All Other Persons			
AFDC and Food Stamps	2.8	11.9	21.1
Medicaid	0.0	4.7	6.7
All Other Programs	17.5	19.7	41.1
Total	$ 67.9	$ 163.8	$ 287.8
Share of Total Spending			
Medicare - Medicaid	.000	.171	.195
Other Programs for the Elderly	.667	.578	.517
Other Programs for the Totally Disabled	.034	.059	.073
AFDC and Food Stamps received by non-aged, non-disabled	.041	.073	.073
Other Programs for Non-Aged, Non-Disabled	.258	.120	.143
	1.000	1.000	1.000

Source: David Ellwood and Lawrence Summers, "Poverty in America: Is Welfare the Answer or the Problem?" Institute for Research on Poverty, Williamsburg Conference Paper, December 1984.

Table 4

Welfare, Female Headship, and Unemployment

Year	Real AFDC Plus Food Stamp Guarantee	Percentage of Nonaged Families with Children Headed by Women	Unemployment Rate, Civilian Workers	
			Nonwhite Men, 18-19 Years	All Men
	(1)	(2)	(3)	(4)
1960	$6715	n.a.	25.1 %	5.4 %
1964	6604	n.a.	23.1	4.6
1968	7129	10.7 %	19.0	2.9
1972	8894	13.8	26.3	5.0
1976	8743	16.7	33.8	7.1
1980	7486	19.8	32.6	6.9
1984	6955	20.8[a]	n.a.	7.4

Sources:

Column (1): Weighted average of states' AFDC and Food Stamp benefit level for a family of four with no income (in 1984 dollars) from Background Material and Data on Programs Within the Jurisdiction of the Committee on Ways and Means, February 22, 1985, p. 532.

Column (2): Computations by authors from March Current Population Survey data tapes.

Column (3): Employment and Training Report of the President, 1982, p. 196.

Column (4): Economic Report of the President, 1985, p. 271.

[a]For 1983; 1984 data is not yet available.

n.a. = not available

there were no reversals in the trends of either family composition or work effort.

Consider all households with children headed by a person under 65 years of age. The percentage of these households headed by women increased steadily from 10.7 to 20.8 percent between 1968 and 1983 (Column 2). As David T. Ellwood and Mary Jo Bane conclude "welfare simply does not appear to be the underlying cause of the dramatic changes in family structure of the past few decades."[4]

Likewise, neither unemployment rates for young black men nor for all men (Columns 3 and 4) correlate with welfare benefit levels. Between 1960 and 1968, when benefits were rising, unemployment was falling; between 1972 and 1980, when benefits were falling, rates were rising. Only the 1968 to 1972 period, in which both benefits and unemployment rates rose, fits the Murray view.

Black teenage unemployment moves closely with the unemployment rate for all men--both fell from 1960 to 1968 and rose thereafter. As with the pretransfer poverty data, this reinforces our view that deteriorating macroeconomic conditions, not increased transfers, account for the problems of the poor.

While the time series we have reviewed are suggestive, they do not resolve the debate about the relative importance of poor economic performance or the disincentive effects of transfers in explaining the trend in poverty. There is, however, an extensive microeconomic literature that

shows that the magnitudes of the labor supply and family structure effects of transfers are much smaller than those required to confirm Murray's thesis on the poverty-increasing effects of social programs.

Robert Moffitt summarizes the experimental income maintenance studies by stating that a $20 per week increase (in 1984 dollars) in the income guarantee will lead a female family head to work 1.8 fewer hours per week.[5] This implies that the $2,179 annual increase (in 1984 dollars) in AFDC and Food Stamps between 1960 and 1972 (shown in Table 4) would have decreased weekly work effort by 2.2 hours. The $1,939 decrease in annual guarantee between 1972 and 1984 would have produced a 2.0 hour increase. Neither of these figures strike us as being sufficiently large to warrant eliminating AFDC and Food Stamps.

Murray cites the same studies summarized by Moffitt and concludes that the experiments demonstrated the "disastrous" effects of income transfers. Yet he does not acknowledge that the experiments provided much higher benefits than existing programs and that increased transfers can only increase posttransfer poverty if the reduction in labor supply more than offsets the increased transfers or if sufficient additional poor households form in response to the benefits. None of the estimated effects are anywhere near large enough to produce his result.

That the work effort of women heading households is not very sensitive to changes in welfare program parameters has been confirmed by the recent experience with the AFDC rule changes enacted under the Omnibus Budget Reconciliation Act of 1981 (OBRA). While many economists expected that the increase in the marginal benefit reduction rate in AFDC to 100

percent would lead to large reductions in labor supply, numerous studies found this not to be the case.[6] Working recipients did not stop working and nonworking recipients did not reduce their rate of entrance into the labor force. Recipients continued to work either to gain useful skills or to signal future employers that they are employable. As a result, the main effect of welfare is to raise the incomes of the poor.

What about male-headed families? AFDC-UP covers unemployed fathers with children in about half of the states, but accounts only for 8.3 percent of the caseload. Applying Moffitt's labor supply estimates for husband-wife families to the 1960-1972 growth in benefits yields a combined reduction in hours worked by husbands and wives of only 2.5 hours per week. Likewise, single men who are eligible only for Food Stamps would have reduced their labor supply by small amounts because of the increased Food Stamp guarantee. None of these estimated responses supports the position that transfers fail to reduce poverty.

Welfare's disincentives have been relatively small because benefits have never been very high relative to full-time work at the minimum wage. This is not to say that the enactment of a comprehensive income support system which substantially increased benefits and extended coverage to previously uncovered groups would not have larger disincentives. It is partly for this reason that we argue below that welfare reform for the able-bodied should rely more on work requirements and the provision of work opportunities and less on increased income transfers.

Holding family composition constant, then, it is clear that the growth in income transfers has substantially reduced poverty. But how

much of the increased percentage of households with children headed by women can be attributed to welfare? Ellwood and Bane use a variety of cross-section and time-series comparisons and find only small effects on birth rates to unmarried women and divorce and separation patterns among families with children. They do find that AFDC has a large effect on the probability that young single mothers live independently rather than with parents or other relatives.[7] This negative consequence of welfare can be remedied by changing the rules so that minors not living with their parents are ineligible for AFDC. Such a change has been proposed by the Reagan administration. However, even if the proportion of persons living in households headed by single women with children had remained constant at the 1967 level, poverty in 1980 would have been lower by only about 0.6 percentage point (12.4 instead of 13.0 percent of all persons). Such an effect does not justify abolishing AFDC.

SOME ANTIPOVERTY REFORMS

Congressional actions over the last two years indicate that Murray's view is not widely accepted--a wholesale elimination of income transfer programs was not considered a viable option. While Congress did enact reductions in many programs, it stopped far short of dismantling the legacies of the War on Poverty and the Great Society.

This of course does not mean that voters or policy analysts or politicians believe that the previous or current structure of social programs is optimal. The major problem with transfer programs is not their disincentives. They ease the plight of the poor and reduce

measured poverty, but they do not provide expanded job opportunities so that the able-bodied can earn their way out of poverty.

We believe that antipoverty policy must be redirected toward increasing the earnings of the able-bodied poor. Welfare recipients have shown themselves willing to work when jobs are available. For example, the Supported Work Demonstration project proved cost-effective for long-term AFDC recipients and early findings from Community Work Experience projects in many states have been promising.

With unemployment rates expected to remain above 6 percent for the remainder of the decade, the disadvantaged are not likely to find sufficient employment and training opportunities in the private labor market. A reorientation of poverty policy that integrates welfare and work for the able-bodied is therefore essential. Let us turn to such a program.

First, the income support system should include a cash assistance program for those who cannot work and a jobs program for those who can. The work ethic is so prevalent that taxpayers and recipients alike prefer work opportunities to welfare for the able-bodied. In keeping with this view, President Carter's welfare reform proposal included a public jobs component and President Reagan has advocated Community Work Experience programs, better known as "workfare."

While some workfare programs seek only to reduce the number of recipients, this need not be the case. The issue should not be whether able-bodied persons must earn their income but rather how much income they can earn. Under many workfare programs, a recipient can work only until she/he has earned an amount equal to the welfare benefit. But if the

program allows the recipient to work full-time, workfare becomes a work opportunity program even if the jobs provide no training.

Many studies suggest that employment programs have had little impact on the beneficiaries' rate of earnings growth or post-program hours of work. This is, however, not the proper criterion for evaluating the employment component of a welfare reform. Its objective is to reduce current income poverty without increasing welfare dependency. Any raises in future earnings are an added benefit.

The second component focuses on the special needs of long-term welfare recipients. Although they are a small minority of persons on the AFDC program at any time, they account for a disproportionate share of program costs and have a disproportionate impact on public perceptions about recipients. The fact that most AFDC recipients leave the program after two to three years shows that welfare generally does not create Murray's poverty trap. Those on the program for long spells should be viewed as having special problems limiting their mobility and should be enrolled in a program that takes account of their special needs.

Our third component aims to increase the incomes of those working for low wages in regular jobs. The Earned Income Tax Credit (EITC) currently gives families with children who earn less than $5,000 annually an extra eleven cents for every dollar earned. This avoids many of the problems associated with direct labor market interventions by subsidizing earnings through the tax system and targeting funds on the working poor. The EITC should be extended to childless families and single individuals.

Our reform requires work but provides work opportunities; it rewards work and reduces poverty without increasing dependency; it builds

on what we have learned of the strengths and weaknesses of previous policies. By helping the poor help themselves, we would achieve the goals of welfare recipients and taxpayers alike.

SUMMARY

Unlike Murray, we find no evidence that increased social spending has increased poverty. The fastest growing and largest expenditures were targeted on the elderly and were clearly successful. And because the work and family disincentives of welfare have been small, its main effect has been to raise the incomes of the poor. We should reduce these disincentives, but not by discarding programs. Rather, we should promote work requirements which are matched with work opportunities.

If current policy remains in effect, the official poverty rate in the next few years will remain above 13 percent even if the current recovery proceeds as expected. Following Murray's proposals would raise the rate even higher.

Notes

[1]Meg Greenfield, Newsweek, February 11, 1985, p. 80.

[2]The Public Interest, Winter 1983, pp. 118-119.

[3]Robert Greenstein, in "Losing Faith in Losing Ground," New Republic, March 25, 1985, challenges Murray's description of these choices.

[4]David T. Ellwood and Mary Jo Bane, "The Impact of AFDC on Family Structure and Living Arrangements." Report prepared for the U.S. Department of Health and Human Services. Mimeo., Harvard University, 1984.

[5]Robert Moffitt, "The Negative Income Tax: Would It Discourage Work?" Monthly Labor Review, 104 (April 1981), 23-27.

[6]Robert Hutchens, "The Effects of OBRA on AFDC Recipients: A Review," Institute for Research on Poverty Discussion Paper No. 764-84, University of Wisconsin-Madison.

[7]Ellwood and Bane, Summary of Findings, p. 2.